British Association for the Advancement of Science.

MANCHESTER MEETING, 1915.

THE PRESIDENT'S ADDRESS

AND THE

SECTIONAL ADDRESSES

LONDON:
OFFICES OF THE ASSOCIATION,
BURLINGTON HOUSE, W.

Price Two Shillings

British Association for the Advancement of Science.

MANCHESTER, 1915.

ADDRESS

BY

PROFESSOR ARTHUR SCHUSTER, D.Sc., Sc.D., LL.D., Dr. ès Sc., F.R.S.,
PRESIDENT.

The Common Aims of Science and Humanity.

UNDER the influence of the diversity of pursuits imposed upon us by the conditions of modern life, different groups of the community—men of business, men of science, philosophers, or artists—have acquired detached and sometimes opposing interests. Each group, impressed by the importance of its own domain in the life of the nation, and focussing its vision on small differences and temporary rivalries, was in danger of losing the sense of mutual dependence. But in the shadow of a great catastrophe it has been brought home to us that the clash of interests is superficial, and the slender thread of union which remained has grown into a solid bond. What is the fibre from which the bond is twined? Patriotism may express its outward manifestation, but its staple is the mental relationship which remains continuous and dominant even in normal times, when each of us may peacefully go to earn his living and enjoy the course of his intellectual life.

Outwardly the community is divided into heterogeneous elements with mental attitudes cast in different moulds, and proceeding along separate roads by differing methods to different ideals. Yet as we eliminate the superficial, and regard only the deep-seated emotions which control our thoughts and actions, the differences vanish, and the unity of purpose and sentiment emerges more and more strongly. Mind and character, no doubt, group themselves into a number of types, but the cleavage runs across, and not along, the separating line of professions.

Were it otherwise, the British Association could not perform one of its most important functions—a function not, indeed, originally contemplated, but resulting indirectly from the wise and democratic provisions in its constitution, which enabled it to adapt itself to the changing needs of the time. Our founders primarily considered the interests of scientific men; their outlook was restricted and exclusive, both as regards range

of subject and membership. In the words of Sir David Brewster, who gave the first impulse to its formation, it was to be 'an Association of our nobility, clergy, gentry, and philosophers.'

The meetings were intended to promote personal intercourse, to organise research, to advocate reform of the laws hindering research, and to improve the status of scientific men. The right of membership was confined to those who already belonged to some learned society, and William Whewell, one of the principal supporters of the movement, even suggested that only authors of memoirs published by a learned society should be admitted.[1] He emphasized this proposal by the recommendation[2] 'in some way to avoid the crowd of lay members whose names stand on the List of the Royal Society.' The reform of the Patent Laws and the introduction of an International Copyright were suggested as subjects suitable for discussion, not apparently from the point of view of general advantage, but merely in the interests of one section of the community.

Whatever the objects of the founders of the Association may have been, it is obvious that questions of public importance could not be permanently excluded from meetings the success of which depended on the interest stimulated in the community. The Statistical Section, which owed its origin to the visit, at the first Oxford Meeting (1836), of Quetelet, the Belgian astronomer and economist, was the first to assert itself by engaging in a discussion of the Poor Laws. Whewell deeply resented this violation of academic neutrality: 'it was impossible,' he wrote, 'to listen to the Proceedings of the Statistical Section on Friday without perceiving that they involved exactly what it was most necessary and most desired to exclude from our Proceedings,'[3] and again: 'Who would propose (I put it to Chalmers, and he allowed the proposal to be intolerable) an ambulatory body, composed partly of men of reputation and partly of a miscellaneous crowd, to go round year by year from town to town and at each place to discuss the most inflammatory and agitating questions of the day?'[4]

Fortunately for our Association, this narrow-minded attitude did not prevail, and our records show that while not avoiding controversial and even inflammatory subjects, we have been able to exercise a powerful influence on the progress of science. The establishment of electric units, universally accepted throughout the world, originated in the work of one of our committees; the efforts which led to the foundation of the National Physical Laboratory, one of the most efficient and beneficial

[1] Others were allowed to join on recommendation by the General Committee. It was only in 1906 that this restriction, which had become obsolete, was removed.

[2] *Whewell's Writings and Letters*, vol. ii. p. 128.

[3] *loc. cit.*, p. 289.

[4] It is much to be desired that the documents relating to the early history of the British Association should be published in a collected form.

organisations in the country, received its first impulses from us; and the organisation of the first world service for the systematic investigation of earth tremors was established by the late Dr. Milne, working through one of our Committees.

The success of these enterprises alone is sufficient to show that we are not merely a body promoting social intercourse between men of science and the rest of the community. Nevertheless, it may be admitted that our efforts have been spasmodic, and the time has arrived to consider whether it may be possible to secure not only a greater continuity in our work but also its better co-ordination with that of other scientific organizations. The present juncture affords the opportunity, and the changed conditions, which in the near future will affect all our institutions, render it indeed incumbent upon us once more to adapt ourselves to the needs of the times. Proposals for a move in that direction have already been made, and will no doubt be carefully considered by the Council. In the meantime, I may draw your attention to the important discussions arranged for by our Economic Section, which alone will justify the decision of the Council not to suspend the Meeting this year.

It must not be supposed that, even in the early days of the Association, Whewell's ideas of its functions were universally accepted. It is pleasant to contrast the lamentations of the omniscient Professor of Mineralogy with the weightier opinion of the distinguished mathematician who then held Newton's chair at Cambridge. At the concluding session of the second meeting of the Association, Babbage expressed the hope 'that in the selection of the places at which the annual meetings were to be held, attention should be paid to the object of bringing theoretical science in contact with the practical knowledge on which the wealth of the country depends.' 'I was myself,' he said, 'particularly anxious for this, owing as I do a debt of gratitude for the valuable information which I have received in many of the manufacturing districts, where I have learned to appreciate still more highly than before the value of those speculative pursuits which we follow in our academical labours. I was one of those who thought at first that we ought to adjourn for our next meeting to some large manufacturing town; but I am now satisfied that the arrangement which has been made will be best adapted to the present state of the Association. When, however, it shall be completely consolidated I trust we may be enabled to cultivate with the commercial interests of the country that close acquaintance which I am confident will be highly advantageous to our more abstract pursuits.'

Since then, as we all know, our most successful meetings have been held in manufacturing centres; but it is important to note that,

while Babbage laid stress on the benefit which would accrue to pure science by being brought into contact with practical life, scientific men of the present day have more and more insisted on the services they, on their part, are able to render to the industries. The idealistic motive has thus given way to the materialistic purpose. Both aspects are perhaps equally important, but it is necessary to insist, at the present time, that the utilitarian drum can be beaten too loudly. There is more than one point of contact between different activities of the human mind, such as find expression in scientific pursuits or commercial enterprises, and it is wrong to base the advantages to be derived from their mutual influence solely, or even mainly, on the ground of material benefits.

I need not press this point in a city which has given many proofs that a business community may be prompted by higher motives than those which affect their pockets. It was not for utilitarian objects that repeated efforts were made since the year 1640 to establish a University in Manchester; it was not for reasons of material gain that the Royal Institution and Owens College were founded; nor was it because they increased the wealth of the district that the place of honour in our Town Hall has been given to Dalton and Joule.

When we glance at the various occupations of the working parts of a nation, comprising the student who accumulates or extends knowledge, the engineer who applies that knowledge, the geologist or agriculturalist who discloses the store of wealth hidden in the soil, the commercial man who distributes that wealth, it seems as if we ought to be able to name the qualities of intellect and temperament which in each pursuit are most needed to carry out the work successfully. But on trying to define these qualities we soon discover the formidable nature of the task. Reasoning power, inventive power, and sound balance of judgment are essential attributes in all cases, and the problem is reduced to the question whether there are different varieties of the attributes which can be assigned to the different occupations.

Among all subjects mathematics is perhaps the one that appears most definitely to require a special and uncommon faculty. Yet, Poincaré—himself one of the clearest thinkers and most brilliant exponents of the subject—almost failed when he attempted to fix the distinguishing intellectual quality of the mathematician. Starting from the incontrovertible proposition that there is only one kind of correct reasoning, which is logical reasoning, he raises the question why it is that everybody who is capable of reasoning correctly is not also a mathematician, and he is led to the conclusion that the characterising feature is a peculiar type of memory. It is not a better memory, for some mathematicians are very forgetful, and many of them cannot add

a column of figures correctly; but it is a memory which fixes the order in which the successive steps of reasoning follow each other without necessarily retaining the details of the individual steps. This Poincaré illustrates by contrasting the memory of a chess-player with that of a mathematician. 'When I play chess,' he says, 'I reason out correctly that if I were to make a certain move, I should expose myself to a certain danger. I should, therefore, consider a number of other moves, and, after rejecting each of them in turn, I should end by making the one which I first contemplated and dismissed, having forgotten in the meantime the ground on which I had abandoned it.' 'Why, then,' he continues, 'does my memory not fail me in a difficult mathematical reasoning in which the majority of chess-players would be entirely lost? It is because a mathematical demonstration is not a juxtaposition of syllogisms, but consists of syllogisms placed in a certain order; and the order in which its elements are placed is much more important than the elements themselves. If I have this intuition—so to speak—of the order, so as to perceive at one glance the whole of the reasoning, I need not fear to forget its elements: each of these will take its right place of its own accord without making any call on my memory.' [5]

Poincaré next discusses the nature of the intellectual gift distinguishing those who can enrich knowledge with new and fertile ideas of discovery. Mathematical invention, according to him, does not consist in forming new combinations of known mathematical entities, because the number of combinations one could form are infinite, and most of them would possess no interest whatever. Inventing consists, on the contrary, in excluding useless combinations, and therefore: 'To invent is to select—to choose.' . . . 'The expression "choose" perhaps requires qualifying, because it recalls a buyer to whom one offers a large number of samples which he examines before making his choice. In our case the samples would be so numerous that a lifetime would not suffice to complete the examination. That is not the way things are done. The sterile combinations never present themselves to the mind of the inventor, and even those which momentarily enter his consciousness, only to be rejected, partake something of the character of useful combinations. The inventor is therefore to be compared with an examiner who has only to deal with candidates who have already passed a previous test of competence.'

All those who have attempted to add something to knowledge must recognize that there is a profound truth in these remarks. New ideas may float across our consciousness, but, selecting the wrong ones for more detailed study, we waste our time fruitlessly. We are bewildered by the multitude of roads which open out before us, and, like Poincaré

[5] *Science et Méthode*, pp. 46 and 47.

when he tries to play chess, lose the game because we make the wrong move. Do we not all remember how, after the announcement of a new fact or generalization, there are always many who claim to have had, and perhaps vaguely expressed, the same idea? They put it down to bad luck that they have not pursued it, but they have failed precisely in what, according to Poincaré, is the essence of inventive power. It may be bad luck not to have had a good idea, but to have had it and failed to appreciate its importance is downright incapacity.

An objection may be raised that before a selection can be made the ideas themselves must appear, and that, even should they arrive in sufficient numbers, the right one may not be among them. It may even be argued that Poincaré gives his case away by saying that 'the sterile combinations do not even present themselves to the mind of the inventor,' putting into a negative form what may be the essence of the matter. Moreover, a fertile mind like that of Poincaré would be apt to place too low a value on his own exceptional gifts. Nevertheless, if Poincaré's more detailed exposition be read attentively, and more especially the description of how the discoveries which made him famous among mathematicians originated in his mind, it will be found that his judgment is well considered and should not be lightly set aside. New ideas seldom are born out of nothing. They most frequently are based on analogies, or the recollection of a sequence of thoughts suggested by a different branch of the subject, or perhaps by a different subject altogether. It is here that the memory comes in, which is not a memory of detail, but a memory of premises with their conclusions, detached from the particular case to which they were originally applied. Before we pronounce an adverse opinion on Poincaré's judgment, we must investigate what constitutes novelty in a new idea, but the subject is too vast to be dealt with here, nor can I attempt to discuss whether an essential distinction exists between mathematical invention and that more practical form of invention with which, for instance, the engineer has to deal.

If Poincaré, by this introspective analysis of his own powers, has dimmed the aureole which, in the eyes of the public, surrounds the mathematician's head, he removes it altogether by his definition of mathematics. According to him, 'mathematics is the art of calling two different things by the same name.' It would take me too far were I to try to explain the deep truth expressed in this apparently flippant form: physicists, at any rate, will remember the revolution created in the fundamental outlook of science by the application of the term 'energy' to the two quite distinct conceptions involved in its subdivisions into potential and kinetic energy.

Enough has been said to show that the peculiar powers necessary

for the study of one of the most abstract branches of knowledge may be expressed in terms which bring them down to the level at which comparison with other subjects is possible. Applying the same reasoning to other occupations, the same conclusion is inevitable. The commercial man, the politician, and the artist must all possess the type of memory best suited to concentrate in the field of mental vision their own experiences as well as what they have learned from the experience of others; and, further, they must have the power of selecting out of a multitude of possible lines of action the one that leads to success; it is this power which Poincaré calls the inventive faculty.

The argument must not be pushed too far, as it would be absurd to affirm that all differences in the capability of dealing successfully with the peculiar problems that occur in the various professions may be reduced to peculiarities of memory. I do not even wish to assert that Poincaré's conclusions should be accepted without qualification in the special case discussed by him. What is essential, to my mind, is to treat the question seriously, and to dismiss the vague generalities which, by drawing an artificial barrier between different groups of professions, try to cure real or imaginary defects through plausible though quite illusory remedies. All these recommendations are based on the fallacy that special gifts are associated with different occupations. Sometimes we are recommended to hand over the affairs of the nation to men of business; sometimes we are told that salvation can only be found in scientific methods—what is a man of business, and what is a scientific method? If you define a man of business to be one capable of managing large and complicated transactions, the inference becomes self-evident; but if it be asserted that only the specialized training in commercial transactions can develop the requisite faculties, the only proof of the claim that could be valid would be the one that would show that the great majority of successful statesmen, or political leaders, owed their success to their commercial experience. On the other hand, every method that leads to a correct result must be called a scientific method, and what requires substantiating is that scientific training is better than other training for discovering the correct method. This proof, as well as the other, has not been, and, I think, cannot be, given. When, therefore, one man calls for the conduct of affairs 'on business lines' and the other clamours for scientific methods, they either want the same thing or they talk nonsense. The weak point of these assertions contrasting different classes of human efforts is that each class selects its own strongest men for comparison with the weakest on the other side.

The most fatal distinction that can be made is the one which brings men of theory into opposition to men of practice, without regard to the obvious truth that nothing of value is ever done which does not involve

both theory and practice; while theory is sometimes overbearing and irritating, there are among those who jeer at it some to whom Disraeli's definition applies: the practical man is the man who practises the errors of his forefathers. With refined cruelty Nemesis infects us with the disease most nearly akin to that which it pleases us to detect in others. It is the most dogmatic of dogmatics who tirades against dogma, and only the most hopeless of theorists can declare that a thing may be right in theory and wrong in practice.

Why does a theory ever fail, though it may be sound in reasoning? It can only do so because every problem involves a much larger number of conditions than those which the investigator can take into account. He therefore rejects those which he believes to be unessential, and if his judgment is at fault he goes wrong. But the practical man will often fail for the same reason. When not supported by theoretical knowledge he generalizes the result of an observation or experiment, applying it to cases where the result is determined by an altogether different set of conditions. To be infallible the theorist would have to take account of an infinite number of circumstances, and his calculations would become unmanageable, while the experimenter would have to perform an infinite number of experiments, and both would only be able to draw correct conclusions after an infinite lapse of time. They have to trust their intuition in selecting what can be omitted with impunity, and, if they fail, it is mainly due to the same defect of judgment. And so it is in all professions: failure results from the omission of essential considerations which change the venue of the problem.

Though theory and practice can only come into opposition when one of them is at fault, there is undoubtedly a contrast in character and temperament between those who incline more towards the one and those who prefer the other aspect: some like a solitary life at the desk, while others enjoy being brought into contact with their fellows. There have at all times been men predestined by nature to be leaders, and leadership is required in all branches of knowledge—the theoretical as well as the more active pursuits; but we must guard against accepting a man's estimate of his own power to convert his thoughts into acts. In the ordinary affairs of life a man who calls himself a man of action is frequently only one who cannot give any reasons for his actions. To claim that title justly a man must act deliberately, have confidence in his own judgment, sufficient tenacity of purpose to carry it through, and sufficient courage to run the unavoidable risks of possible failure. These risks may be trivial or they may be all-important. They may affect the reputation of one unit of creation or involve the whole life of a nation, and according to the greatness of the issue we shall honour

the man who, having taken the risk, succeeds. But whether the scale be microscopic or interstellar, the essence of the faculty of blending theory and practice is the same, and both men of books and men of action are to be found in the philosopher's study and the laboratory, as well as in the workshop or on the battlefield. Modern science began, not at the date of this or that discovery, but on the day that Galileo decided to publish his Dialogues in the language of his nation. This was a deliberate act destined to change the whole aspect of science which, ceasing to be the occupation of a privileged class, became the property of the community. Can you, therefore, deny the claim of being a man of action to Galileo, can you deny it to Pasteur, Kelvin, Lister, and a host of others? There are, no doubt, philosophers who cannot manage even their own affairs, and whom it would be correct to call pure theorists, but that proves nothing, because their defect makes them worse philosophers as well as worse citizens.

In his Presidential Address, delivered to this Association in 1899, Sir Michael Foster summarized the essential features of the scientific mind. Above all other things he considered that its nature should be such as to vibrate in unison with what it is in search of; further, it must possess alertness, and finally moral courage. Yet after enumerating these qualities, he arrives at the same result which I have tried to place before you, that there are no special peculiarities inherent in the scientific mind, and he expresses this conclusion in the following words:

"'But, I hear some one say, these qualities are not the peculiar attributes of the man of science, they may be recognized as belonging to almost everyone who has commanded or deserved success, whatever may have been his walk in life. That is so. That is exactly what I would desire to insist, that the men of science have no peculiar virtues, no special powers. They are ordinary men, their characters are common, even commonplace. Science, as Huxley said, is organized common-sense, and men of science are common men drilled in the ways of common-sense.'

This saying of Huxley's has been repeated so often that one almost wishes it were true, but unfortunately I cannot find a definition of common-sense that fits the phrase. Sometimes the word is used as if it were identical with *uncommon* sense, sometimes as if it were the same thing as common *nonsense*. Often it means untrained intelligence, and in its best aspect it is, I think, that faculty which recognizes that the obvious solution of a problem is frequently the right one. When, for instance, I see, during a total solar eclipse, red flames shooting out from the edge of the sun, the obvious explanation is that these are real phenomena caused by masses of glowing vapours ejected from the sun; and when a learned friend tells me that all this is an

optical illusion due to anomalous refraction, I object on the ground that the explanation violates my common-sense. He replies by giving me the reasons which have led him to his conclusions, and, though I still believe that I am right, I have to meet him with a more substantial reply than an appeal to my own convictions. Against a solid argument common-sense has no power and must remain a useful but fallible guide which both leads and misleads all classes of the community alike.[6]

If we must avoid assuming special intellectual qualities when we speak of groups of men within one country, we ought to be doubly careful not to do so without good reason in comparing different nations. So-called national characteristics are in many cases matters of education and training; and, if I select one as an example, it is because it figures so largely in public discussions at the present moment. I refer to that expedient for combining individual efforts which goes by the name of organization. An efficient organization requires a head that directs and a body that obeys; it works mainly through discipline, which is its most essential attribute. Every institution, every factory, every business establishment is a complicated organism, and no country ever came to prominence in any walk of life unless it possessed the ability to provide for the efficient working of such organisms. To say that a nation which has acquired and maintained an Empire, and which conducts a large trade in every part of the world, is deficient in organizing power is therefore an absurdity. Much of the current self-depreciation in this respect is due to the confusion of what constitutes a true organization with that modification of it which to a great extent casts aside discipline and substitutes co-operation. Though much may be accomplished by co-operation, it is full of danger in an emergency, for it can only work if it be loyally adhered to; otherwise it resembles a six-cylinder motor in which every sparking-plug is allowed to fix its own time of firing. Things go well so long as the plugs agree; but there is nearly always one among them that persists in taking an independent course and, when the machine stops, complains that the driver is inefficient. The cry for organization, justifiable as it no doubt often is, resolves itself, therefore, into a cry for increased discipline, by which I do not mean the discipline enforced at the point of the bayonet, but that accepted by the individual who voluntarily subordinates his personal convictions to the will of a properly constituted authority. This discipline is not an inborn quality which belongs more to one nation than to another; it is acquired by education and training. In an emergency it is essential to success, but if it be made the guiding

[6] Since writing the above, I find on reading Professor J. A. Thomson's 'Introduction to Science' a similar criticism of Huxley's dictum. Prof. Thomson's general conclusions are not, however, in agreement with those here advocated.

principle of a nation's activity, it carries dangers with it which are greater than the benefits conferred by the increased facility for advance in some directions.

If there be no fundamental difference in the mental qualifications which lead to success in our different occupations, there is also none in the ideals which move us in childhood, maintain us through the difficulties of our manhood, and give us peace in old age. I am not speaking now of those ideals which may simultaneously incite a whole nation to combined action through religious fervour or ambition of power, but I am speaking of those more individual ideals which make us choose our professions and give us pleasure in the performance of our duties.

Why does a scientific man find satisfaction in studying Nature?

Let me once more quote Poincaré [7]:—

'The student does not study Nature because that study is useful, but because it gives him pleasure, and it gives him pleasure because Nature is beautiful; if it were not beautiful it would not be worth knowing and life would not be worth living. I am not speaking, be it understood, of the beauty of its outward appearance—not that I despise it, far from it, but it has nothing to do with science: I mean that more intimate beauty which depends on the harmony in the order of the component parts of Nature. This is the beauty which a pure intelligence can appreciate and which gives substance and form to the scintillating impressions that charm our senses. Without this intellectual support the beauty of the fugitive dreams inspired by sensual impressions could only be imperfect, because it would be indecisive and always vanishing. It is this intellectual and self-sufficing beauty, perhaps more than the future welfare of humanity, that impels the scientific man to condemn himself to long and tedious studies. And the same search for the sense of harmony in the world leads us to select the facts which can most suitably enhance it, just as the artist chooses among the features of his model those that make the portrait and give it character and life. There need be no fear that this instinctive and unconscious motive should tempt the man of science away from the truth, for the real world is far more beautiful than any vision of his dreams. The greatest artists that ever lived—the Greeks—constructed a heaven; yet how paltry that heaven is compared to ours! And it is because simplicity and grandeur are beautiful that we select by preference the simplest and grandest facts, and find our highest pleasure, sometimes in following the gigantic orbits of the stars, sometimes in the microscopic study of that minuteness which also is a grandeur, and sometimes in piercing the secrets of geological times

[7] *Loc. cit.* p. 15.

which attract us because they are remote. And we see that the cult of the beautiful guides us to the same goal as the study of the useful.'

'Whence comes this harmony? Is it that things that appear to us as beautiful are simply those which adapt themselves best to our intelligence, and are therefore the tools which that intelligence handles most easily; or is it all the play of evolution and natural selection? In that case, those races only survived whose ideals best conformed with their interests, and while all nations pursued their ideals without regard to consequences, some were led to perdition and others achieved an empire. One is tempted to believe that such has been the course of history, and that the Greeks triumphed over the barbarians, and Europe, inheritor of Greek thought, rules the world, because the savages cared only for the sensual enjoyment of garish colours and the blatant noise of the drum, while the Greeks loved the intellectual beauty which is hidden beneath the visible beauty. It is that higher beauty which produces a clear and strong intelligence.' If the mathematician's imagination is fired by the beauty and symmetry of his methods, if the moving spring of his action is identical with that of the artist, how much truer is this of the man of science who tries by well-designed experiments to reveal the hidden harmonies of Nature? Nor would it be difficult, I think, to trace the gratification inherent in the successful accomplishments of other intellectual pursuits to the same source.

Though Poincaré was, I believe, the first to lay stress on the connexion between the search for the beautiful and the achievement of the useful, the æsthetic value of the study of science had previously been pointed out, and well illustrated, by Karl Pearson in his "Grammar of Science.' As expressed by him: 'it is this continual gratification of the æsthetic judgment which is one of the chief delights of pure science.' Before we advance, however, any special claim for the pursuit of science based on these considerations, we must pause to think whether they do not equally apply to other studies or occupations. For this purpose, the nature of the æsthetic enjoyment involved must be remembered. We do not mean by it, the pleasure we feel in the mere contemplation of an impressive landscape or natural beauty, but it resembles more the enjoyment experienced on looking at a picture where, apart from the sensual pleasure, we are affected by the relation between the result of the representation and that which is represented. The picture, quite apart from what it may be trying to imitate, has a certain beauty due to its contrast of colours or well-balanced arrangement. We have in one case a number of pigments covering a space of two dimensions, and in the other the natural object in three dimensions made up of entirely different materials and showing an infinite variety

of detail and appearance. By itself alone either a mere photographic representation, or a geometrical arrangement of colour and line, leaves most of us cold; though both have their own particular beauty, the art consists in bringing them into connexion. Bearing in mind the æsthetic value of the relationship of the work of our brain or hand to external facts or appearances, it might easily be shown that what has been said of science equally applies to other studies, such as history or literature. We may even go further, and say that any occupation whatever, from which we can derive an intellectual pleasure, must possess to a greater or smaller degree the elements of combining the useful with the beautiful.

In order to trace in detail the part played by purely emotional instincts in directing the course of our lives, we should have to study the causes which influence a child, free to select his future profession. Having eliminated secondary effects, such as early associations, or the personal influence of an inspiring teacher, we should probably be brought to a standstill by the dearth of material at our disposal, or led into error by taking our own individual recollections as typical. Nevertheless it is only through the record of each man's experience that we may hope to arrive at a result. If every man who has reached a certain recognized position in his own subject—it need not be pre-eminence—would write down his own recollections of what led him to make the choice of his profession, we might hope to obtain facts on which a useful psychological study might be based. Scientific men as a class are not modest, but they share with other classes the reluctance to speak of their early life, owing to a certain shyness to disclose early ambitions which have not been realized. It requires courage to overcome that shyness, but I think that we need feel no shame in revealing the dreams of our childhood and holding fast to them despite the bondage of our weakness, despite the strife ending so often in defeat, despite all the obstacles which the struggle for existence has placed in our path. In some form they should persist throughout our lives and sustain us in our old age.

But the account of our early life should be simple, detached from any motives of self-depreciation or self-assertion, and free from any desire to push any particular moral or psychological theory. We want to trace the dawn of ambition, the first glimmering in the child's mind that there is something that he can do better than his fellows and reminiscences of early likes and dislikes which, though apparently disconnected from maturer tendencies, may serve as indications of a deep-seated purpose in life. It may be difficult to resist the temptation of trying to justify one's reputation in the eyes of the world; but it is worth making the effort. The only example that I know of such an

autobiographical sketch is that of Darwin, which is contained in his 'Life and Letters,' published by his son, Sir Francis Darwin.

The ambition of a child to be better, cleverer, or more beautiful than its fellows is in the main, I think, a wish to please and to be praised. As the child grows up, the ambition becomes more definite. It is not a sordid ambition for ultimate wealth or power, nor is it an altruistic ambition to do good for the sake of doing good. Occasionally it takes the form confessed to by Darwin, when he says: 'As a child I was much given to inventing deliberate falsehoods, and this was always done for the sake of causing excitement.' This desire to be conspicuous was, in Darwin's case, consistent with extreme modesty, amounting almost to a want of confidence in himself, as appears in this passage: 'I remember one of my sporting friends, Turner, who saw me at work with my beetles, saying that I should some day be a Fellow of the Royal Society, and this notion seemed to me to be preposterous.'

We next come to the stage where a child is attracted by one subject more than another, and, if his choice be free, will select it for his life's career. What guides him in this choice? If it be said that a boy gravitates towards that subject which he finds easiest, we are led to the further question why does he find it easiest? It is on this point that more information is required, but I am inclined to answer in accordance with Poincaré's views that it is because its particular beauty appeals most strongly to his emotional senses. In questions of this kind everyone must form his own conclusions according to his personal recollections, and these convince me that the emotional factor appears already at an early age. It is the strong attraction towards particular forms of reasoning, more perhaps even than the facility with which reasoning comes, that carries us over the initial difficulties and the drudgery that must accompany every serious study.

I have already alluded to the different tendencies of individuals either to prefer solitary reflexion or to seek companionship. Almost in every profession we find men of both types. Darwin's autobiography furnishes a good example of the man who prefers to learn through quiet reading rather than through lectures, but to many men of science the spoken word is inspiring and contact with congenial minds almost a necessity.

From our present point of view the most interesting passages in Darwin's autobiography are those indicating the æsthetic feeling which, like Poincaré, he connects with scientific research. Referring to his early studies we find this passage: 'I was taught by a private tutor and I distinctly remember the intense satisfaction which the clear geometrical proofs gave me. I remember with equal distinctness the delight which my uncle gave me by explaining the principle of the

vernier of a barometer.' To a man who apparently had no pronounced facility of mastering mathematical difficulties this feeling of satisfaction is especially remarkable. The combination of scientific ability with leanings either to music, or art, or poetry, is very common, and examples are to be found in almost every biography of men of science. It is difficult indeed to name an eminent scentific man who has not strong leanings towards some artistic recreation: we find the poetic vein in Maxwell and Sylvester, the musical talent in Helmholtz and Rayleigh, and the enthusiastic though amateurish pictorial efforts of less important men. That the similarities are to be found also in temperament may be noticed on reading Arnold Bennett's article on 'The Artist and the Public',[8] where many passages will be seen to be applicable to students of science as well as to writers of fiction.

If we look for distinctions between different individuals, we may find one in their leanings either towards the larger aspects of a question or the microscopic study of detail. The power of focussing simultaneously the wider view and the minute observation is perhaps the most characteristic attribute of those who reach the highest eminence in any profession, but the great majority of men have a notable predilection for the one or other side. Though it is indispensable for a scientific man to study the details of the particular problem he is trying to solve, there are many who will lose interest in it as soon as they believe they can see a clear way through the difficulties without following up their solution to its utmost limits. To them detail, as such, has no interest, and they will open and shut a door a hundred times a day without being even tempted to inquire into the inner working of the lock and latch.

There is only one feature in the operation of the intelligence by means of which a sharp division may possibly be drawn between brainworkers showing special capabilities in different subjects. In some persons thought attaches itself mainly to language, in others to visualised images, and herein lies perhaps the distinction between the literary and scientific gift. Those who, owing to external circumstances, have resided in different countries are sometimes asked in what language they think. Speaking for myself, I have always been obliged to answer that, so far as I can tell, thought is not connected with any language at all. The planning of an experiment or even the critical examination of a theory is to me entirely a matter of mental imagery, and hence the experience, which I think many scientific men must have shared, that the conversion of thought into language, which is necessary when we wish to communicate its results to others, presents not only the ordinary difficulties of translation but reveals faults in the perfection or sequence of the images. Only when the logic of words finally

[8] *English Review*, October 1913.

coincides with the logic of images do we attain that feeling of confidence which makes us certain that our results are correct.

A more detailed examination of the instinctive predilections of a child would, I think, confirm Poincaré's conclusion that a decided preference for one subject is in the main due to an unconscious appeal to his emotions. It should be remembered, however, that the second step of Poincaré's philosophy is as important as the first. The mere emotional impulse would die out quickly, if it were not supplemented by the gratification experienced on discovering that the search for the beautiful leads us to results which satisfy our intellect as well as our emotions. There may still be bifurcations in the second portion of the road. Some may rest content with achieving something that supplies the material needs of humanity, others may be inspired to search for the deeper meaning of our existence.

There remains therefore some justification for the question why we persist in studying science apart from the mere intellectual pleasure it gives us. It was once a popular fallacy to assume that the laws of Nature constituted an explanation of the phenomena to which they applied, and people then attached importance to the belief that we could gauge the mind of the Creator by means of the laws which govern the material world, just as we might trace the purpose of a human legislator in an Act of Parliament. As this archaic interpretation was abandoned, philosophers went, in accordance with what politicians call the swing of the pendulum, to the other extreme. We can explain nothing, they said—in fact, we can know nothing—all we can do is to record facts. This modesty was impressive and it became popular. I know, at any rate, one scientific man who has acquired a great reputation for wisdom by repeating sufficiently often that he knows nothing, and, though his judgment may be true, this frame of mind is not inspiring. As a corrective to the older visionary claims, which centred round the meaning of the word 'explain', the view that the first task of science is to record facts has no doubt had a good influence. Kirchhoff laid it down definitely that the object of science is to describe Nature, but he did not thereby mean that it should be confined to recording detached observations: this would be the dullest and most unscientific procedure. Description, in the sense in which Kirchhoff uses it, consists in forming a comprehensive statement gathering together what, till then, was only a disconnected jumble of facts. Thus the apparently quite irregular motions of the planets, as observed from the earth, were first collected in tabular form. This was a necessary preliminary but was not in itself a scientific investigation. Next came Kepler, who by means of three laws summed up the facts in their main outlines, and the description then took a more refined form, sub-

stituting half a page of printing for volumes of observations. Finally, Newton succeeded in predicting the planetary movements on the assumption of a gravitational attraction between all elements of matter. According to Kirchhoff, the chief merit of this discovery would lie in its condensing Kepler's three laws into one hypothesis. This point of view is not necessarily opposed to that of Poincaré, because it is exactly the simplicity of Newton's explanation that appeals most strongly to our æsthetic sense, but there is an important difference in the manner of expression. However beautiful an idea may be, it loses its effect by being placed before us in an unattractive form. This criticism also applies to Mach, according to whom the object of science is to economize thought, just as it is the object of a machine to economize effort. Logically, this definition is justified, and it may be the best that can be given, if we prefer using a technical expression to confessing an emotional feeling. But why should we do so? Is it not better to recognize that human intelligence is affected by sentiment as much as by reasoning? It is a mistake for scientific men to dissociate themselves from the rest of humanity, by placing their motives on a different, and, at the best, only superficially higher, level. When an adventurous spirit, for instance, desires to organize an expedition to unknown regions of the world, we try to induce our governments to provide the necessary funds by persuading them, and incidentally ourselves, that we do so because important scientific results may be expected from the expedition. This may actually be the case, but we are mainly affected by the same motives as the rest of the community: if the truth be told, we are as curious as they to know what every corner of the earth looks like, and we join them in wishing to encourage an enterprise requiring perseverance and involving danger.

I fully realize that the wish to justify one's own work in the eyes of the world will always lead to fresh attempts to find a formula expressing the objects which we desire to attain. Enough, however, has been said to show that the definition must take account of sentiment, without insisting too much upon it. Nor can we hope, in view of the variety of intellectual and emotional pleasures which combine to create the charm of science, to include all points of view, but if I were forced to make a choice I should say that the object of science is to predict the future. The wish to know what lies before us is one of the oldest and most enduring desires of human nature; often, no doubt, it has degenerated and given rise to perverted and ignoble longings, but its accomplishment, when it can be achieved by legitimate inquiry, is a source of the purest and most satisfying enjoyment that science can give. We feel that enjoyment each time we repeat an old and perhaps

hackneyed experiment. The result is known beforehand, but be it only that we expect the colour of a chemical precipitate to be green or yellow, be it only that we expect a spot of light to move to the right or left, there is always a little tremor of excitement at the critical moment and a satisfying feeling of pleasure when our expectation has been realized. That pleasure is, I think, enhanced when the experiment is not of our own making but takes place uncontrolled by human power. In one of Heine's little verses he makes light of the tears of a young lady who is moved by the setting sun. 'Be of good cheer,' the poet consoles her, 'this is only the ordinary succession of events: the sun sets in the evening and rises in the morning.' If Heine had been a man of science, he would have known that the lady's tears found a higher justification in the thought of the immutable and inexorable regularity of the sun's rising and setting than in the fugitive colour impression of his descent below the horizon, and that her emotions ought to be intensified rather than allayed by the thought of his resurrection in the morning—everybody's life contains a few unforgettable moments which, at quite unexpected times, will vividly rise in his mind, and there are probably some in this Hall who have experienced such moments at the beginning of a total eclipse of the sun. They have probably travelled far, and gone through months of preparation, for an event which only lasts a few minutes. The time of first contact is approaching, in a few seconds the moon is about to make its first incision in the solar disc, and now the observer's thoughts come crowding together. What if there were a mistake in our calculations? What if we had chosen a spot a few miles too far north or too far south? What if the laws of gravitation were ever so little at fault?—But now at the predicted time, at the calculated spot on the sun's edge, the dark moon becomes visible, and the feeling of relief experienced concentrates into one tense instant all the gratitude we owe those who have given precision to the predictions of celestial movements, leaving them expressible by a simple law which can be written down in two lines. It is this simplicity of the law of gravitation, and its accuracy, which some day may show limitations, but has hitherto withstood all tests, that gives to Astronomy its pre-eminence over all sciences.

Indeed, if we classify the different sections into which science may be divided, I think it may be said that their aim, in so far as it is not purely utilitarian, is always either historic or prophetic; and to the mathematician, history is only prophecy pursued in the negative direction. It is no argument against my definition of the objects of science, that a large section of its sub-divisions has been, and to some extent still is, mainly occupied with the discovery and classification of

facts; because such classification can only be a first step, preparing the way for a correlation into which the element of time must enter, and which therefore ultimately must depend either on history or prophecy.

Latterly men of science, and in particular physicists, have given increased attention to the intrinsic meaning of the concepts by means of which we express the facts of Nature. Everything—who can deny it?—is ultimately reduced to sense impressions, and it has therefore been asserted that science is the study of the mind rather than of the outside world, the very existence of which may be denied. The physicist has thus invaded the realm of philosophy and metaphysics, and even claims that kingdom as his own. Two effects of these efforts, a paralyzing pessimism and an obscure vagueness of expression, if not of thought, seriously threatened a few years ago to retard the healthy progress of the study of Nature. If the outside world were only a dream, if we never could know what really lies behind it, the incentive which has moved those whose names stand out as landmarks in science is destroyed, and it is replaced by what? By a formula which only appeals to a few spirits entirely detached from the world in which they live. Metaphysicians and physicists will continue to look upon science from different points of view, and need not resent mutual criticisms of each other's methods or conclusions. For we must remember that most of the good that is done in this world is done by meddling with other people's affairs, and though the interference is always irritating and frequently futile, it proves after all that our interests converge towards a common centre.

According to Poincaré, the pleasure which the study of science confers consists in its power of uniting the beautiful with the useful; but it would be wrong to adopt this formula as a definition of the object of science, because it applies with equal force to all human studies. I go further, and say that the combination of the search for the beautiful with the achievement of the useful is the common interest of science and humanity. Some of us may tend more in one direction, some in another, but there must always remain a feeling of imperfection and only partial satisfaction unless we can unite the two fundamental desires of human nature.

I have warned you at the beginning of this discourse not to beat the utilitarian drum too loudly, and I have laid stress throughout on the idealistic side, though the most compelling events of the moment seem to drive us in the other direction, and the near future will press the needs of material prosperity strongly upon us. I must guard myself, therefore, against one criticism which the trend of my remarks may invite. At times, when the struggle for existence keeps masses in permanent bondage, in a society in which a multitude of men and

women have to face starvation, and when unfortunate, though purely accidental, surroundings in childhood drive the weak into misery, is it not futile to speak of æsthetic motives? Am I not, while endeavouring to find a common bond between all sections of the community, in reality drawing a ring round a small and privileged leisured class, telling them these enjoyments are for you and for you alone? Should I not have found a surer ground for the claims of science in its daily increasing necessity for the success of our manufactures and commerce?

I have said nothing to indicate that I do not put the highest value on this important function of science, which finds its noblest task in surrendering the richness of its achievements to the use of humanity. But I must ask you to reflect whether the achievement of wealth and power, to the exclusion of higher aims, can lead to more than a superficial prosperity which passes away, because it carries the virus of its own doom within it. Do we not find in the worship of material success the seed of the pernicious ambition which has maddened a nation, and plunged Europe into war? Is this contempt for all idealistic purposes not responsible for the mischievous doctrine that the power to possess confers the right to possess, and that possession is desirable in itself without regard to the use which is made of it? I must therefore insist that if we delight in enlisting the wealth accumulated in the earth, and all the power stored in the orbs of heaven, or in the orbits of atomic structure, it should not be because we place material wealth above intellectual enjoyment, but rather because we experience a double pleasure if the efforts of the mind contribute to the welfare of the nation. When Joule taught us to utilize the powers at our disposal to the best advantage he did it not—and his whole life is a proof of it—to increase either his own wealth or that of the nation, but because, brought up in commercial life and deeply imbued with the deep insight and genius of science, he found his greatest delight in that very combination of æsthetic satisfaction and useful achievement which Poincaré has so well described. And again, when another of our fellow-citizens, Henry Wilde, shewed how electrical power can be accumulated until it became an efficient instrument for the economic transmission of work, he found his inspiration in the intellectual gratification it gave him, rather than in the expectation of material gain. I am drawing no ring round a privileged class, but urge that the hunger for intellectual enjoyment is universal and everybody should be given the opportunity and leisure of appeasing it. The duty to work, the right to live, and the leisure to think are the three prime necessities of our existence, and when one of them fails we only live an incomplete life.

I should have no difficulty in illustrating by examples, drawn from

personal experience, the power which the revelations of science can exert over a community steeped in the petty conflicts of ordinary life; but I must bring these remarks to a conclusion, and content myself with the account of one incident.

An American friend, who possessed a powerful telescope, one night received the visit of an ardent politician. It was the time of a Presidential election, Bryan and Taft being the opposing candidates, and feeling ran high. After looking at clusters of stars and other celestial objects, and having received answers to his various questions the visitor turned to my friend:

'And all these stars I see,' he asked, 'what space in the heaven do they occupy?'

'About the area of the moon.'

'And you tell me that every one of them is a sun like our own?'

'Yes.'

'And that each of them may have a number of planets circulating round them like our sun?'

'Yes.'

'And that there may be life on each of these planets?'

'We cannot tell that, but it is quite possible that there may be life on many of them.'

And after pondering for some time, the politician rose and said: 'It does not matter after all whether Taft or Bryan gets in.'

Happy were the times, when it could be said with truth that the strife of politics counted as nothing before the silent display of the heavens. Mightier issues are at stake to-day: in the struggle which convulses the world, all intellectual pursuits are vitally affected, and Science gladly gives all the power she wields to the service of the State. Sorrowfully she covers her face because that power, accumulated through the peaceful efforts of the sons of all nations, was never meant for death and destruction; gladly she helps, because a war wantonly provoked threatens civilization, and only through victory shall we achieve a peace in which once more Science can hold up her head, proud of her strength to preserve the intellectual freedom which is worth more than material prosperity, to defeat the spirit of evil that destroyed the sense of brotherhood among nations, and to spread the love of truth.

British Association for the Advancement of Science.

SECTION A: MANCHESTER, 1915.

ADDRESS

TO THE

MATHEMATICAL AND PHYSICAL SCIENCE SECTION

BY

SIR F. W. DYSON, M.A., LL.D., F.R.S.,

PRESIDENT OF THE SECTION.

ALTHOUGH at the present time our minds are largely absorbed by the war the meeting of the British Association in Manchester indicates that we consider it right to make our annual review of scientific progress. I shall therefore make no apology for choosing the same subject for my address as I should have chosen in other circumstances. It is a subject far removed from war, being an account of the manner in which astronomers have with telescopes and spectroscopes investigated the skies and the conclusions they have reached on what Herschel called 'The Construction of the Heavens.'

Our knowledge of the fixed stars, as they were called by the old astronomers, is of comparatively recent origin, and is derived from two sources: (1) the measurement of small changes in the positions of the stars in the sky, and (2) the analysis of the light received from them and the measurement of its amount. The facts found for separate stars when arranged and classified give us some insight into the structure of the Stellar Universe as a whole. The discovery made by Galileo's telescope that the Milky Way consists of myriads of stars, may be taken as the beginning of Sidereal Astronomy. The further study of the number of the stars and their general distribution has grown with the light-grasping powers of the telescope, and in recent times has had the assistance of photography.

The changes of position of the stars among themselves are of two kinds. The first consist of small periodic movements about a mean position due to parallax, and in the case of double stars due to orbital motion. The observation of these small angles has been made possible by the development of the telescope for refined measurement. The most important contributions to this are the Equatorial movement, the position micrometer, the heliometer, and the application of photography. For the purpose of measurement the increase in focal length and the perfect optical definition of the telescope are of greater importance than increase in light-grasping power.

The second class of movements, the proper motions of stars, are determined by the comparison of the positions of the stars after an interval of years. The accurate positions of stars in the sky are found by means of the transit-circle and the astronomical clock. Both of these instruments have been slowly brought to a high degree of perfection. The use of photography makes it possible to extend the study of proper motions to the fainter stars.

Accurate measures of the light of the stars have been in progress for the last fifty years by the applications of photometers of various kinds to the telescope. Many observations of magnitude, especially of variable stars, have

also been made by estimation, but are brought to a uniform scale by comparison with photometric measures. In the last few years photography has been very successfully applied to determine stellar magnitudes, and seems likely to supersede visual observations to a large extent.

By spectroscopic analysis the stars may be classified according to their physical characteristics, and their velocities in the line of sight may be determined. For the purposes of classification, objective prisms have been generally employed, and the spectra of many stars obtained on one photographic plate. The measurement of velocities in the line of sight is a more delicate operation, and although initiated in the 'sixties, it was not till the application of photography thirty years later that results of value were obtained. This class of observation requires a large telescope and a spectroscope very carefully designed and constructed.

This very brief summary of the different kinds of observations made in the study of the stars may remind you to what extent progress has been dependent on the development of astronomical instruments. The desire to examine fainter objects, and still more the necessity of increasing the accuracy of observations, has brought about a continuous improvement in the range and accuracy of astronomical instruments. Methods which had been perfected for observations of a few stars have been extended so that they can be applied to a large number. For these reasons the progress of Sidereal Astronomy may seem to have gone on slowly for a time. The more rapid progress of recent years arises from the accumulation of data, for which we are indebted to generations of astronomers, and from the gradual increase in power and perfection of our instruments.

The first insight into the stars as a whole naturally came from the survey of their numbers and distribution; and Herschel, who constructed the first great telescopes, explored the heavens with untiring skill and energy, and speculated boldly on his observations, is justly regarded as the founder of Sidereal Astronomy. In his great paper, 'On the Construction of the Heavens,' Herschel gives the rules by which he was guided, which I should like to quote, as they may well serve as a motto to all who are engaged in the observational sciences :

'But first let me mention that if we would hope to make any progress in an investigation of this delicate nature we ought to avoid two opposite extremes of which I can hardly say which is the most dangerous. If we indulge a fanciful imagination and build worlds of our own, we must not wonder at our going wide from the path of truth and nature; but these will vanish like the Cartesian vortices, that soon gave way when better theories were offered. On the other hand, if we add observation to observation, without attempting to draw not only certain conclusions but also conjectural views from them, we offend against the very end for which only observations ought to be made. I will endeavour to keep a proper medium; but if I should deviate from that I could wish not to fall into the latter error.' In this spirit he discussed the 'star gauges' or counts of stars visible with his great reflector in different parts of the sky, and concluded from them that the stars form a cluster which stretches to an unknown but finite distance, considerably greater in the plane of the Milky Way than in the perpendicular direction. He gave this distance as 497 times that of Sirius. He did not hesitate to advance the theory that some of the nebulæ were similar clusters of stars, of which that in Andromeda, judging from its size, was the nearest. Herschel had no means of telling the scale of the sidereal system, though he probably supposed the parallax of Sirius to be of the order of 1″.

Though some of the assumptions made by Herschel are open to criticism, the result at which he arrived is correct in its general outline. I shall attempt to give a brief account of some of the principal methods used to obtain more definite knowledge of the extent and constitution of this 'island universe.' The stars of which most is known are, in general, those nearest to us. If the distance of a star has been measured, its co-ordinates, velocity perpendicular to the line of sight and luminosity are easily found. In the case of a double star whose orbit is known the mass may also be determined. But only a very small proportion of the stars are sufficiently near for the distance to be determin-

able with any accuracy. Taking the distance corresponding to a parallax of 1″ or the parsec as unit—*i.e.*, 200,000 times the distance of the Earth from the Sun—fairly accurate determinations can be made up to a distance of 25 parsecs, but only rough ones for greater distances.

For much greater distances average results are obtainable from proper motions, and the mean distances of particular classes of stars—for instance, stars of a given magnitude or given type of spectrum—can be found with confidence up to a distance of 500 parsecs, and with considerable uncertainty to twice this distance. The density of stars in space as a function of the distance, the percentage of stars within different limits of luminosity, the general trend of the movements of stars and their average velocities can also be found, within the same limits of distance.

For all distances, provided the star is sufficiently bright, its velocity to or from the earth can be measured. The general consideration of these velocities supplies complementary data which cannot be obtained from proper motions, and confirms other results obtained by their means.

For distances greater than 1,000 parsecs our knowledge is generally very vague. We have to rely on what can be learned from the amount and colour of the light of the stars, and from their numbers in different parts of the sky.

Parallax.

Let us begin with the portion of space nearest to us, within which the parallaxes of stars are determinable. The successful determination of stellar parallax by Bessel, Struve, and Henderson in 1838 was a landmark in Sidereal Astronomy. The distances of three separate stars were successfully measured, and for the first time the sounding line which astronomers had for centuries been throwing into space touched bottom.

The employment of the heliometer which Bessel introduced was the main source of our knowledge of the distances of stars till the end of the nineteenth century, and resulted in fairly satisfactory determination of the parallaxes of less than 100 stars.

Nineteen stars have been found to be within a sphere of radius 5 parsecs, or a million times the Sun's distance. We cannot say that all the stars within this distance have been discovered, but there are good reasons to think that most of them have been found. Leaving out stars of very faint luminosity—less than $\frac{1}{250}$th part of the luminosity of the Sun—Prof. Eddington estimates the total number in this volume as thirty-two. This gives that in the space near the Sun the average distance of the stars from one another is $2\frac{1}{2}$ to 3 parsecs—or twice the distance of the Sun from its nearest neighbour α Centauri.

A considerable proportion of these stars are double and the orbits of several have been determined. The distance being known, the linear dimensions of the orbit are immediately determined and the masses. From these somewhat scanty data it is found that there is not a great range in the masses of stars. Thus the combined mass of Sirius and its companion is three and a half times that of the Sun, and the total mass of α Centauri is twice that of the Sun. These results are confirmed statistically by observations of spectroscopic binary stars and of other double stars. There is no evidence of any stars with masses a hundred times greater than the Sun or of any with much smaller masses. According to Prof. Russell, the largest stellar mass of which we know is the spectroscopic binary and eclipising variable star V Puppis, and this is 19 times as massive as the Sun. Further, it seems, as has been pointed out by Ludendorf and Halm, that the bright helium stars are the most massive, being on the average seven times as massive as the Sun.

When the absolute luminosities of the stars whose distances have been measured are calculated, it is found that, unlike the masses, they exhibit a very great range. For example, Sirius radiates forty-eight times as much light as the Sun and Groombridge 34 only one hundredth part. This does not represent anything like the complete range, and Canopus, for example, may be ten thousand times as luminous as the Sun. But among the stars near the Solar system, the absolute luminosity appears to vary with the type of spectrum. Thus Sirius, of type A, a blue hydrogen star, is forty-eight

times as luminous as the Sun; Procyon of type F5—bluer than the Sun but not so blue as Sirius—ten times; α Centauri, which is nearly of Solar type, is twice as luminous. 61 Cygni of type K5—redder than the Sun—one-tenth as luminous; while the still redder star of type Ma, Gr 34, is only one-hundredth as luminous. In the neighbourhood of the Solar system one-third of the stars are more luminous and two-thirds less luminous than the Sun. The luminosity decreases as the type of spectrum changes from A to M, *i.e.*, from the blue stars to the red stars.

These three results as to the density in space, the mass and the luminosity have been derived from a very small number of stars. They show the great value of accurate determinations of stellar parallax. As soon as the parallax is known, all the other observational data are immediately utilisable. At the commencement of the present century the parallaxes of perhaps 80 stars were known with tolerable accuracy. Happily the number is now rapidly increasing by the use of photographic methods. Within the last year or two, the parallaxes of nearly two hundred stars have been determined and published. This year a Committee of the American Astronomical Society, under the presidency of Prof. Schlesinger, has been formed to co-ordinate the work of six or seven American and one or two English observatories. The combined programme contains 1,100 stars, of which 400 are being measured by more than one observatory. We may expect results at the rate of two hundred a year, and may therefore hope for a rapid increase of our knowledge of the stars within our immediate neighbourhood.

Velocities in the Line of Sight.

The determination of radial velocities was initiated by Huggins in the early 'sixties, but reliable results were not obtained till photographic methods were introduced by Vogel in 1890. Since that time further increase in accuracy has been made, and the velocity of a bright star with sharp lines is determinable (apart from a systematic error not wholly explained) with an accuracy of ¼ kilomètre per second. As the average velocities of these stars are between 10 and 20 kilomètres a second, the proportional accuracy is of a higher order than can be generally obtained in parallax determinations or in other data of Sidereal Astronomy. A number of observatories in the United States and Europe, as well as in South America, the Cape, and Canada are engaged in this work. Especially at the Lick Observatory under Prof. Campbell's direction, the combination of a large telescope, a well-designed spectroscope, and excellent climatic conditions have been utilised to carry out a bold programme. At that observatory, with an offshoot at Cerro San Christobal in Chile, for the observation of stars in the Southern Hemisphere, the velocities of 1,200 of the brightest stars in the sky have been determined. Among the results achieved is a determination of the direction and amount of the Solar motion. The direction serves to confirm the results from proper motions, but the velocity is only obtainable accurately by this method. This quantity which enters as a fundamental constant in nearly all researches dealing with proper motion, is given by Campbell at 19·5 kilomètres per second, or 4·1 times the distance of the Earth from the Sun per annum, though there is some uncertainty arising from a systematic error of unknown origin.

Variation of their radial velocity shows that a large proportion of stars are spectroscopic binaries, and the results have been discussed by Campbell from the point of view of the genesis of the double stars by fission. It would be somewhat outside the scope of my address to speak further of this. I have already drawn attention to the fact that we derive from spectroscopic binary stars a considerable part of our somewhat scanty knowledge of the masses of stars.

The observations of radial velocities have shown within what limits the velocities of stars lie and have given a general idea of their distribution. The most important result, and one of a somewhat surprising character, is that the mean velocities of stars, the motion of the Sun being abstracted, increase with the type of spectrum. Thus the stars of type B, the helium stars, the stars of the highest temperature, have average radial velocities of only 6·5 kilomètres per second; the hydrogen stars of type A have average velocities of

11 kilomètres per second; the Solar stars of 15 kilomètres per second; while for red stars of types K and M it has increased slightly more to 17 kilomètres per second. Further, the few planetary nebulæ—*i.e.*, condensed nebulæ with bright line spectra—have average velocities of 25 kilomètres per second. There can be no question of the substantial accuracy of these results, as they are closely confirmed by discussions of proper motions. They are, however, very difficult to understand. On the face of it, there does not seem any reason why stars of a high temperature should have specially high velocities. A suggestion has been thrown out by Dr. Halm that as the helium stars have greater masses, these results are in accordance with an equi-partition of Energy. But the distances of stars apart is so great that it seems impossible that this could be brought about by their interaction. Prof. Eddington suggests that the velocities may be an indication of the part of space at which the stars were formed (*e.g.*, stars of small mass in outlying portions), and represents the kinetic energy they have acquired in arriving at their present positions.

The stars whose radial velocities have been determined are, generally speaking, brighter than the fifth magnitude. Fainter stars are now being observed. At the Mount Wilson Observatory, Prof. Adams has determined the velocities of stars of known parallaxes, as there are great advantages in obtaining complete data for stars where possible. Extension of line-of-sight determinations to fainter stars is sure to bring a harvest of useful results, and a number of great telescopes are engaged, and others will shortly join in this important work.

Proper Motions.

As proper motions are determined by the comparison of the positions of stars at two different epochs, they get to be known with constantly increasing accuracy as the time interval increases. The stars visible to the naked eye in the Northern Hemisphere were accurately observed by Bradley in 1755. Many thousands of observations of faint stars down to about $9^{m}{\cdot}0$ were made in the first half of the nineteenth century. An extensive scheme of reobservation was carried out about 1875 under the auspices of the Astronomische Gesellschaft. A great deal of reobservation of stars brighter than the ninth magnitude has been made this century in connection with the photographic survey of the heavens. For the bright stars all available material has been utilised, and their proper motions have been well determined, and for the fainter stars this is being gradually accomplished.

Proper motions differ widely and irregularly in amount and direction. Herschel observed a tendency of a few stars to move towards one point of the sky, and attributed this sign of regularity to a movement of the Solar system in the opposite direction. As the amount of material increased, the question was examined in different ways by Bessel, Argelander, and Airy. Bessel's method did not indicate the Solar motion, while Airy's showed it plainly. The cause of this discrepancy was not explained for more than half a century. The publication by Auwers of very accurate proper motions of the stars observed by Bradley, consisting roughly of 3,200 stars visible to the naked eye in these latitudes, caused a number of astronomers to make fresh determinations of the direction of the Solar motion. But the puzzling differences given by different methods remained unexplained till the difficulty was resolved by Prof. Kapteyn in a paper read before this Section of the British Association at its meeting in South Africa ten years ago. He showed that the proper motions had a general tendency towards two different points of the sky and not towards one only, as would be expected if the motions of the stars themselves were haphazard, but viewed from a point in rapid motion. He concluded from this that there was a general tendency of the stars to stream in two opposite directions. It is interesting to notice that this great discovery was made by a simple graphical examination of the proper motions of stars in different regions of the sky, after the author had spent much time in examining and criticising the different methods which had been adopted for the determination of the direction of the Solar motion.

The subject was brought into a clearer and more exact shape by the analytical formulation given to it by Prof. Eddington. He employed the

proper motions of some 4,000 stars determined from the comparison of Groombridge's observations in 1810 with modern observations at Greenwich. These stars are more suitable than Bradley's for analytical treatment, as there are a larger number of them per unit area of the sky. This analytical treatment was modified by Prof. Schwarzschild, who considered the stars, not as two separate streams, but as exhibiting a polarity in their proper motions. It is difficult to say which of the two harmonises better with the observations—they agree in the most essential fact, that the stars have a very decided preference for motion towards a point in the Milky Way situated in the constellation of Ophiuchus and the opposite point in the constellation of Orion.

This star-streaming is corroborated by observations of velocities in the line of sight. It applies—with the exception of the helium stars—to all stars which are near enough for their proper motions to be determinable. We may say with certainty that it extends to stars at distances of two or three hundred parsecs; it may extend much further, but I do not think we have at present much evidence of this. Prof. Turner pointed out that the convergence of proper motions did not necessarily imply movements in parallel directions, and suggested that the star-streams were movements of stars to and from a centre. The agreement of the radial velocities with the proper motions seems to me to be opposed to this suggestion, and to show that star-streaming indicates approximate parallelism in two opposite directions in the motions of the stars examined. As the great majority of these stars are comparatively near to us, it is possible that this parallelism is mainly confined to them, and indicates the general directions of the orbital motions of stars in the neighbourhood. An attempted explanation on these lines, as on Prof. Turner's, implies that the Sun is some distance from the centre of the stellar system.

A discovery of an entirely different character was made by Prof. Boss in 1908. He spent many years in constructing a great catalogue giving the most accurate positions and motions of 6,200 stars obtainable from all existing observations. This catalogue, which was published by the Carnegie Institute, was intended as a preliminary to a still larger one which would give the accurate positions and motions of all the stars down to the seventh magnitude. In the introduction to the catalogue, Boss remarks that this collation of the results of meridian observations in a large and comprehensive way is only the second attempt which has yet been made by astronomers. The first, it is interesting to notice, was a general catalogue of 8,377 stars compiled by Francis Baily and published by the British Association in 1845. At that time the proper motions could only be given for a very limited number of stars, but in Boss's catalogue proper motions are given for all the stars, and their probable errors are not more than 0″·5 per century. In the course of this work Professor Boss found that forty or fifty stars scattered over a considerable region of the sky near the constellation Taurus were all moving towards the same point in the sky and with nearly the same angular velocity. He inferred that these stars were all moving in parallel directions with an equal linear velocity, and the supposition was verified, in the case of several of them, by the determination of their radial velocities. From these data he was able to derive the distance of each star and thus its position in space. The existence of a large group of stars, separated from one another by great distances, and all having the same motion in space, is a very remarkable phenomenon. It shows, as was pointed out by Prof. Eddington, how small is the gravitational action of one star on another, and that the movement of each star is determined by the total attraction of the whole mass of the stars. Several other interesting moving clusters have been found since. For all the stars belonging to these clusters, the distances have been found, and from them luminosities and velocities of individual stars, particulars which are generally only obtainable for stars much nearer to us.

Proper motions are the main source of our knowledge of the distances of stars which are beyond the reach of determination by annual parallax. If a star were known to be at rest its distance could be calculated from the shift of its apparent position caused by the translation of the Solar motion. As the Solar system moves 410 times the distance of the Earth from the Sun in a century, this gives a displacement of 1″ for a star at the distance of

500 parsecs. This method has been applied by Kapteyn to determine the distances of the helium stars, as their velocities are sufficiently small to be neglected in comparison with that of the Solar system. But generally it is only possible to find the mean distances of groups of stars of such size that it may be assumed that the peculiar motions neutralise one another in the mean. For example, the average distance of stars of type A, or stars of the fifth magnitude, or any other group desired may be found. In this way Kapteyn has found from the Bradley stars that the mean parallax of stars of magnitude m is given by the formula log. $\pi_m = -1{\cdot}108 - 0{\cdot}125\, m$.

In conjunction with another observational law which expresses the number of stars as a function of the magnitude, this leads to a determination of the density of stars in space at different distances from us, and also of the 'luminosity law,' *i.e.*, the percentage of stars of different absolute brightness. Professors Seeliger and Kapteyn have shown in this way that there is a considerable falling off of star-density as we go further from the Solar system. It seems to me very necessary that this should be investigated in greater detail for different parts of the sky separately. A general mathematical solution of general questions which arise in the treatment of astronomical statistics has been given by Professor Schwarzschild. His investigations are of the greatest value in showing the exact dependence of the density, luminosity, and velocity laws on the statistical facts which can be collected from observation. The many interesting statistical studies which have been made are liable to be rather bewildering without the guidance furnished by a general mathematical survey of the whole position.

When the proper motions are considered in relation to the spectral types of the stars, the small average velocities of the hydrogen stars and still smaller ones of the helium stars found from line-of-sight observations are confirmed. If stars up to a definite limit of apparent magnitude, say, to 6·0 m., or between certain limits, say, 8·0 m. and 9·0 m., are considered, then the Solar stars are found to be much nearer than either the red or the blue stars. Thus both red and blue stars must be of greater intrinsic luminosity than the Solar stars. As regards blue stars, this agrees with results given by parallax observations. But the red stars appear to consist of two classes, one of great and one of feeble luminosity, and it does not seem that a sufficient explanation is given by the fact that a selection of stars brighter than any given apparent magnitude will include the very luminous stars which are at a great distance, but only such stars of feeble luminosity as are very near.

The significance of these facts was pointed out by Prof. Hertzsprung and Prof. Russell. They have a very important bearing on the question of stellar evolution, a subject for discussion at a later meeting this week. From the geometrical standpoint of my address these facts are of importance in that they help to classify the extraordinarily large range found in the luminosities of stars. Putting the matter somewhat broadly, the A stars, or hydrogen stars, are on the average intrinsically 5 magnitudes brighter than the Sun, whilst the range in their magnitudes is such that half of them are within $\frac{3}{4}$ magnitude of the mean value. The stars of type M, very red stars, are of two classes. Some of them are as luminous as the A stars, and have a similar range about a mean value 5 magnitudes brighter than the Sun. Others, on the contrary, have a mean intrinsic brightness 5 magnitudes fainter than the Sun and with the same probable deviation of $\frac{3}{4}$ magnitude. Between the types M and A there are two classes whose distance apart diminishes as the stars become bluer. The facts in support of this contention are very forcibly presented by Prof. Russell in *Nature* in May 1914. If this hypothesis is true, and it seems to me there is much to be said in its favour, then the apparent magnitude combined with the type of spectrum will give a very fair approximation to the distances of stars which are too far away for their proper motions to be determinable with accuracy.

In dealing with the proper motions of the brighter stars, the sky has been considered as a whole. Now that the direction and amount of the solar motion are known, we may hope that, as more proper motions become available, the different parts of the sky will be studied separately. In this way we shall obtain more detailed knowledge of the streaming, and also of the mean

distances of stars of different magnitudes in all parts of the sky, leading to a determination of how the density of stars in space changes in different directions. A second line of research which may be expected to give important results is in the relationship of proper motions to spectral type. There is in preparation at Harvard College by Miss Cannon under Prof. Pickering's direction, a catalogue giving the type of spectrum of every star brighter than the ninth magnitude. It would be very desirable to determine the proper motions of all these stars. If all the material available is examined it should be possible to do this to a very large extent.

Photometry and Colour.

For the more distant parts of the heavens proper motions are an uncertain guide, and we must depend on what can be learned from the light of the stars by means of stellar photometry, determinations of colour, and studies of stellar spectra. Speaking generally, we attempt to discover from the nearer stars sufficient about their intrinsic luminosities to enable us to use the apparent magnitude as an index of the distances of the stars which are further away. The most striking example is found in Prof. Hertzsprung's determination of the distance of the small Magellanic Cloud. Visitors to Australia last year may have seen in the sky two faint patches of light which look like pieces torn off the Milky Way. These are called the Magellanic Clouds. In the small cloud Miss Leavitt found 25 variable stars of special character known as Cepheids. They are all very faint stars between 11·2 m. and 16·4 m. on the photographic scale. The periods of their light variation range from 1·25 days to 127 days. Miss Leavitt found that a linear relationship existed between the logarithm of the period and the apparent magnitude. As these stars all belong to the Magellanic Cloud they are at the same distance, and thus there is a relationship between the period of light variation and the intrinsic magnitude. Prof. Hertzsprung found in Boss's catalogue 13 variable stars of similar class of known proper motion. He deduced their mean distance by using the solar motion, and from this calculated the mean intrinsic luminosity. He thus found that Cepheid variables with a period of 6·6 days are 600 times as luminous as the Sun, and have an absolute magnitude of $-7·3$ m. But from Miss Leavitt's observations similar stars in the small Magellanic Cloud have an apparent visual magnitude of 13·0 m. Thus the small Magellanic Cloud is at such a distance that a star in it is 20·3 m. fainter than it would be if at a distance of one parsec, from which it follows that the distance of this cloud is 10,000 parsecs.

This example illustrates the utility of exact measurements of the light of the stars. Much attention has been given of late years to Stellar Photometry. In 1899 Prof. Pickering published the Revised Harvard Photometry giving the magnitudes of all stars brighter than 6·5 m. In 1907 Messrs. Müller and Kempf completed a determination of 14,199 stars of the Northern Hemisphere brighter than 7·5 m. In 1908 a catalogue of 36,682 stars fainter than 6·5 m. was published at Harvard. These determinations derive additional importance as they give the means of standardising estimates of magnitude made by eye, particularly the many thousands of the Bonn Durchmusterung.

By the labours of Prof. Pickering and his colleagues at Harvard, Prof. Schwarzschild, Prof. Parkhurst at Yerkes, Prof. Seares at Mount Wilson, and others, the determinations of the magnitudes of stars by photography has made rapid strides. As yet no complete catalogues of photographic magnitudes corresponding to the Revised Harvard Photometry have been published, though considerable parts of the sky and special areas such as the Pleiades have been carefully studied. The determination of the photographic magnitudes of any stars which may be required is, however, a comparatively simple matter when the magnitudes of sufficient standard stars have been found. A trustworthy and uniform scale has been to a large extent secured by the use of extrafocal images, gratings, and screens in front of the object glass, and the study of the effects of different apertures and different times of exposure.

At Harvard and Mount Wilson, standard magnitudes of stars near the North Pole have been published extending to nearly the twentieth magnitude. In the part of the range extending from 10·0 m. to 16·0 m. these agree very satis-

factorily. Near the limit at magnitude 20·0 m. there is naturally some discordance, as might be expected, but for the present this is not of great importance. There is, however, a difference of 0·4 m. in the scale between 6·0 m. and 10·0 m. which needs to be cleared up. I may remind you, to make it quite clear what this scale means, that for every increase of 5·0 m. there is a diminution of light in the proportion of 100 : 1. Thus the total range in going from the brightest stars to those of 20·0 m. is more than 10^8 to 1.

A uniform and accurate scale of magnitude is of fundamental importance in counts of the numbers of stars. Such counts aim at the determination of two things: (1) how the numbers vary in different parts of the sky, and (2) what is the ratio of the number of stars of each magnitude to that of the preceding magnitude in the same area of the sky. The counts of stars from the gauges of Sir William and Sir John Herschel, those of the stars contained in the Bonn Durchmusterung, those made by Prof. Celoria, and the recent counts of the Franklin-Adams plates, all agree in showing a continuous increase of stars as we proceed from the pole of the Galaxy to the Galaxy itself. The importance of this fact is that it shows a close connection between the Milky Way and the stars nearer to us. The Milky Way is not a system of stars beyond the others, but is the primary feature of our 'island universe.'

So far there is general agreement. Depending mainly on the counts of Sir John Herschel made at the Cape, and the Cape Photographic Durchmusterung, Prof. Kapteyn finds a very great concentration of faint stars towards the Milky Way. On the other hand, the Bonn Durchmusterung, the counts of Prof. Celoria, and the recent counts of the Franklin-Adams plates by Mr. Chapman and Mr. Melotte give nearly the same concentration, *e.g.*, the proportion of 16·0 m. to 9·0 m. stars does not vary much at different distances from the Galaxy. According to these counts, the total number of stars brighter than 6·0 m. is approximately four times the number brighter than 5·0 m. in all parts of the sky; the number brighter than 9·0 m. is three times the number brighter than 8·0 m.; the number greater than 15·0 m. is double that greater than 14·0 m. From the gradual diminution of the ratio for successive magnitudes, the total number for the whole sky is inferred to be between 1,000 and 2,000 millions, the median coming about the magnitude 23·0 m. The total amount of light received from all the stars is equivalent of 700 or 800 stars of the first magnitude, of which half comes from the stars brighter than 10·0 m. These counts do not by themselves make it possible to determine how the stars fall off in density. But as Prof. Eddington has pointed out, they give a measure of the flattening of the stellar system in the ratio of $3\frac{1}{2}$ to 1. If there is a concentration of faint stars in the Milky Way, as maintained by Prof. Kapteyn, this ratio will be increased.

Photometric observations have acquired additional importance from the differences between photographic and visual magnitudes. The ordinary plate is more sensitive to blue light than the eye, and the difference between the photographic and visual (or photo-visual) magnitude of a star is an index of the colour. The colour index is found by observation to be related very closely to the type of spectrum. Prof. Seares has shown from the Colour Indices that as the stars become fainter they become progressively redder. Prof. Hertzsprung has found the same thing by the use of a grating in front of the object glass. Among stars of 17·0 m. visual magnitude, Seares found none with a colour index less than ·7; this is approximately the colour index of a star of Solar type, *i.e.*, near the middle of the range from blue stars to red stars.

There are three ways in which this may occur. The stars may be bright but very distant red stars; or they may be faint red stars, like those in the immediate neighbourhood of the Sun; or there may have been an absorption of blue light. It is not possible to say in what proportion these causes have contributed. The red stars of 9·0 m and 10·0 m. are nearly all very luminous but distant bodies, but it seems likely that stars of 17·0 m. will contain a greater proportion of stars of small luminosity.

The absorption of light in space is very small and as yet imperfectly determined. Prof. Kapteyn and Mr. Jones, by comparing the colour indices of stars of large and small proper motion, make the difference between the absorption of photographic and visual light as 1 m. in 2,000 parsecs. The question has

been examined directly by Prof. Adams, who has obtained spectra of near and distant stars which are identical as regards their lines, and has examined the distribution of the continuous light. This direct method of comparison showed that the more distant star was always weaker in violet light. But as both these investigations show that very luminous stars are intrinsically somewhat bluer than less luminous stars of the same spectral type, the two causes require further research for their disentanglement. The question is of importance, as it may serve in some cases to determine the distances of very remote bodies whose type of spectrum is known.

It must be admitted that we are as yet very ignorant of the more distant parts of the 'island universe.' For example, we can make little more than guesses at the distance of the Milky Way, or say what part is nearest to us, what are its movements, and so on. But, nevertheless, the whole subject of the Construction of the Heavens has been opened up in a remarkable manner in the last few years. The methods now employed seem competent to produce a tolerably good model showing the co-ordinates and velocities of the stars as well as their effective temperatures and the amount of light they radiate. Industry in the collection of accurate data is required, along with constant attempts to interpret them as they are collected. The more accurate and detailed our knowledge of the stellar system as it is now, the better will be our position for the dynamical and physical study of its history and evolution.

British Association for the Advancement of Science.

SECTION C: MANCHESTER, 1915.

ADDRESS

TO THE

GEOLOGICAL SECTION

BY

PROFESSOR GRENVILLE A. J. COLE, F.G.S., M.R.I.A.,

PRESIDENT OF THE SECTION.

IN his Address to this Section at Sydney in 1914, my predecessor, Sir Thomas Holland, dealt with the problem of isostatic balance in the earth's crust, and with the relation of crust-movements to what Ampferer has styled the *Untergrund*. Such broad questions must appeal to all geologists. Without movements of the surface, the ocean-depths would become diminished by infilling from the denuded lands, and the water would spread, by a general transgression, across the shores of worn-down continents. Rivers would become reduced, both in length and volume, and there would be a marked diminution in the salts carried to the sea. Molluscan life would probably profit by the greater extent of warm and shallow waters, while the variety of animal types on a given land-area would decrease before a growing equality of conditions. Volcanic action, so commonly the accompaniment of large displacements, would no longer find definite lines of outbreak, and a number of interesting petrographic types might remain unseen or even undifferentiated in the quiescent cauldrons of the crust. Students of tectonics, physical geography, palæontology, and petrography are thus alike concerned with superficial warping. More than this, the whole life of man, his future as much as his past, is conditioned by the security or insecurity of the land on which he moves. If to-day I venture to touch on some of these large aspects of our science, it will be understood that this is not from any pride of knowledge on my part, or from any special grasp of a 'theory of the earth.' The conclusion that I should prefer to emphasise is that the faithful and minute observations of the geologist, the discussion of detail, the aid that he may draw from the experiments of the chemist and the physicist, and, above all, the frequent conference with others in the field, all tend to an understanding of human surroundings on this strange rotating globe. The globe is still strange to us, because its vast interior is unseen; and we are apt to speculate about the stars, when the behaviour of the ground beneath us concerns us far more nearly.

Changes in the relative Proportions of Sea and Land.

The geologist has long been accustomed to regard the crust beneath his feet as subject to changes which are immeasurably slow in comparison with the duration of his personal life. Since the days of Lyell, the processes of reconstruction have seemed to us as mild and lengthy as are the processes of decay. It is true that even the latter processes display vigorous tendencies in the form of landslides and the paroxysmic eruptions of volcanoes, while earthquakes at times are accompanied by visible displacements of the crust, such as that which, at Yakutat Bay, in 1899, raised the sea-coast as much as forty-seven feet.

These somewhat exceptional manifestations come well within our conceptions of uniformity, and many of us have felt with Lyell[1] 'that the energy of the subterranean movements has been always uniform as regards the *whole earth*. The force of earthquakes may for a cycle of years have been invariably confined, as it is now, to large but determinate spaces, and may then have gradually shifted its position, so that another region, which had for ages been at rest, became in its turn the grand theatre of action.'

James Hutton has sometimes been charged with catastrophic tendencies, in requiring a complete wearing away of the continents, followed by a somewhat sudden restoration of the land-surface. But he was careful to urge[2] that 'the powers of Nature are not to be employed in order to destroy the very object of those powers; we are not to make Nature act in violation to that order which we actually observe.' To him, the object of the earth's existence was the propagation of life, and particularly of man, upon its surface. We must presume that the destructive outpourings of the lava-rifts of Laki in 1783 and the human hecatomb on the quay at Lisbon in 1755 had not appealed to him as breaks in an orderly succession. He admits[3] that 'this world is thus destroyed in one part, but it is renewed in another; and the operations by which this world is thus constantly renewed are as evident to the scientific eye as are those in which it is necessarily destroyed.' Yet the operations that are to 'give birth to future continents,' as well as those that wear down a continent to the level of the sea, are not the result of 'any violent exertion of power, such as is required in order to produce a great event in little time; in nature, we find no deficiency in respect of time, nor any limitation with regard to power.'[4] Far from believing in the complete loss of the former land-surface before upheaval raised the new, Hutton points out that 'the just view is this, that when the former land of the globe had been complete, so as to begin to waste and be impaired by the encroachment of the sea, the present land began to appear above the surface of the ocean. In this manner we suppose a due proportion to be always preserved of land and water upon the surface of the globe, for the purpose of a habitable world, such as this which we possess.' He then observes that the materials brought to light from the bottom of the sea must have been derived from a land-surface still older than that which is decaying simultaneously with the uprise of new continents. Though he speaks of the strata formed at the bottom of the sea as becoming 'violently bended, broken, and removed from their original place,' he refrains from definite assertions as to the details of the process of elevation.[5] Fusion, whereby original rock-structures are lost, subsequent consolidation, and final upheaval by thermal expansion, seemed to Hutton the broad stages of that part of his cycle which is concerned with reconstruction. He certainly speaks[6] of 'the greatest catastrophes which can happen to the earth, that is, in being raised from the bottom of the sea, and elevated to the summits of a continent, and being again sunk from its elevated station to be buried under that mass of water from whence it had originally come.' But the gist of his whole treatise is that the process of degradation is brought about by slowly acting causes, and the 'catastrophe' of elevation is kept well out of the picture, on the ground that we are unable to follow out its successive stages.

Hutton's insistence on the recurring cycle of geological events was of immense value in checking the imagination of those who revelled in the contemplation of

> 'Crags, knolls, and mounds, confusedly hurl'd,
> The fragments of an earlier world.'

Stellar observation and physical chemistry, however, have alike led us to look for evolutionary processes in the globe; and it is well to ask if the conditions prevalent at the present day are necessarily those of previous periods, or have

[1] *Principles of Geology*, vol. i. (1830), p. 64.
[2] *Theory of the Earth* (1795), vol. ii. p. 547.
[3] *Ibid.* p. 562.
[4] *Ibid.* (1795), vol. i. p. 182.
[5] *Ibid.* pp. 163, 164, 184, and 121.
[6] *Ibid.* vol. ii. p. 445.

even persisted since the surface became suitable for the amatory escapades and internecine enterprises of living organisms.

Few geologists, for instance, will now urge with Hutton that a 'due proportion' has always been preserved between land and water on the surface of the globe, if by those words is meant a proportion such as we now enjoy. We may remark, in passing, that the proportion regarded with complacency by Hutton may have suited the populations and ambitions of the eighteenth century; but recent events have at any rate shown the need for an expansion of the continents.

If we go back to early times, we must consider, with R. A. Daly,[7] the possible grouping of the land against which the Huronian or late pre-Cambrian sediments were formed. The stimulating imagination of this author has proposed a threefold explanation of the absence of calcareous coverings or strengthenings from the organisms of primæval seas. I use the word 'imagination' advisedly, since the power of conceiving what has happened in the past is not necessarily limited by observation of what is now going on around us. The conclusions of Hutton as to the nature of the contact of granite and sediments in Glen Tilt, and those of G. P. Scrope as to the dynamic origin, or at any rate the dynamic intensification, of foliation in crystalline rocks, are triumphs of the imaginative faculty. The geologist who represses his imagination does, perhaps, excellent observational work; but if this repression becomes habitual, others will reap the intellectual harvest of which he has counted out the seeds.

R. A. Daly, then, has imagined, as one of the causes contributing to a 'limeless ocean,' a primitive distribution of land and water very different from that which determines our continental land to-day. His pre-Huronian land-surface is pictured as merely a number of large islands, on which no long and conspicuous rivers could arise. Granitic rocks, moreover, prevailed, and basic materials, capable of supplying calcium in abundance, had not yet become prominent in the surface-layers of the crust.

It may be said that this primitive condition of the distribution of land and water is very unlikely to return. But we have evidence that Hutton's 'due proportion' has been interfered with from time to time. The conversion of the Danish area into islands in the human epoch, and the severance of the British region from continental Europe, are merely pictures in little of what may happen in an unstable crust. The very general spread of the sea over the land-margins in Cenomanian times is attributable to a shallowing of the ocean-floors, and it is difficult to say whether this process has been rhythmic or exceptional in the history of the globe. The Carboniferous period opened with marine conditions over a large part of the northern hemisphere, indicating, not only a continuation of the Devonian seas, but an overflowing of much of the Caledonian land. The same period closes with an extension of the continental edges, and the formation of swampy flats, in which the vegetation of the epoch has been abundantly preserved. Similarly, the sea which deposited the Cretaceous strata, after encroaching alike on South Africa and Scandinavia, withdrew to a considerable extent in Eocene times, and its perpetuation along the Mediterranean belt only calls attention to its subdivision or absence in other areas.

The Foundations of the Earth's Crust.

Hutton, however, remains at present unassailable in one of his most remarkable propositions. He was not troubled by any theory of nebular origins, nor by isogeotherms and their gradual retreat from the surface on which we live. For him, the oldest rocks that we know are sedimentary, and these sediments differed in no respect from those of modern days. This conclusion has perhaps not received the full attention that it deserves. It was based on philosophic reasoning rather than on observation, and its world-wide truth has only recently become appreciated. It now appears certain that we possess no record of a

[7] 'The Limeless Ocean of Pre-Cambrian Time,' *Amer. Journ. Sci.*, vol. xxiii. (1907), p. 113; and more fully in 'First Calcareous Fossils,' *Bull. Geol. Soc. America*, vol. xx. (1909), p. 157.

sedimentary type peculiar to the early stages in the formation of a habitable crust. If such a type existed, it has been lost to us through subsequent metamorphism, amounting to the actual fusion and redistribution of its constituents. The Grenville series of North America, first recognised by Logan, has been studied in considerable detail.[8] Its relation to the Keewatin series of Canada is unknown, but it rests on a floor of granitoid rock, which is intrusive in it, and which belongs to the oldest of various irruptive groups. The Grenville series includes conglomerates, false-bedded quartzites, and a development of limestone that is altogether exceptional for pre-Cambrian times. In Finland,[9] sediments have been traced down to the layer where their original characters vanish in a general 'migmatitic' ground. The Bottnian and the still older Ladogan systems alike provide us with strata in which primary characters have been preserved. Conglomerates and phyllites occur among them, and near Tampere (Tammerfors) the seasonal stratification is as well recorded in a Bottnian shale as it is in the Pleistocene clays made famous in Sweden by De Geer. Vein-gneiss (Ådergneis) underlies these ancient systems, and represents their destruction by the injection of granite from below. It is to be noted that J. J. Sederholm[10] believes that in several places in Finland the 'basement complexes of the typical Archæan sedimentary formations' have been preserved. These, however, may well be also sedimentary, and thus similar in origin to the later complexes above them. Their surfaces must be due to denudation, and their igneous constituents were intruded into rocks which may have been merely a cover worn from still older masses.

If we accept the meteoritic and planetesimal hypotheses of Lockyer and Chamberlin, it is quite possible to argue that the primitive crust was never molten as a whole. It may never have lost its fragmental structure, which was original and due to accretion from without. When agents of denudation came to work upon it, a chemical as well as a mechanical sifting of various materials came about. The oldest sediments were less differentiated than their successors; chemical adjustments may then have been made in response to demands from the interior of the globe[11]; and ultimately normal types of sediment—that is, types to which we are now accustomed—began to gather in hollows of the surface. So far, Hutton's position becomes strengthened by the postulation of an unfused planetesimal crust, and the restriction of molten masses and hydrothermal activity to the interior of a consolidating globe.

The doctrines of Laplace, however, led Hutton's immediate successors to see in crystalline schists the products of abnormal sedimentation. In their view, a molten globe became surrounded by a slowly consolidating crust, and highly heated waters, playing upon this, deposited crystalline material on the floors of primordial seas.[12] Among other workers on pre-Cambrian rocks, T. G. Bonney[13] has upheld the view that the conditions under which schists were formed have not been repeated in later geological times. This is, of course, true if they are deposits from hot solutions and were laid down at the surface of the earth.

R. A. Daly[14] has reasoned that we may accept the planetesimal view and yet believe that a molten surface prevailed at some time over the whole globe; and A. Holmes,[15] in a recent and lucid paper, supports these arguments from the

[8] F. D. Adams and A. E. Barlow, 'Geology of Haliburton and Bancroft Areas,' *Geol. Surv. Canada*, Mem. 6 (1910), p. 36.

[9] J. J. Sederholm, 'Ueber eine archäische Sedimentformation im südwestlichen-Finland,' *Bull. Comm. géol. Finlande*, No. 6 (1899), p. 215.

[10] *Op. cit.* No. 6, p. 213, and 'Om. Granit och Gneis,' *ibid.*, No. 23 (1907), p. 100.

[11] See C. H. L. Schwarz, *Causal Geology* (1910), p. 11.

[12] G. P. Scrope, *Considerations on Volcanos* (1825), p. 226.

[13] Presidential Address, *Quart. Journ. Geol. Soc. London*, vol. xlii. (1886), *Proceedings*, p. 110. See also T. Sterry Hunt, 'Études sur les Schistes cristallins,' *C.R.*, *Congrès géol. internat.* 1888, p. 65.

[14] *Igneous Rocks and their Origin* (1914), p. 159.

[15] 'Radio-activity and the Earth's Thermal History,' *Geol. Mag.* 1915, p. 105. On the power of radio-active substances to promote rock-fusion see, however, J. P. Iddings, *The Problem of Volcanism* (1914), p. 141.

probabilities of radio-active heating. Yet, so far as we have any record left to us, Hutton remains fundamentally in the right. All modern research shows that the schists and gneisses can be explained by causes now in action. The vast majority of schists were at one time normal sediments; others were tuffs or lavas; but, whether originally sedimentary or igneous, they owe their present characters to widely spread regional metamorphism.

The Undermining and Weakening of the Foundations of the Crust.

Is there, then, any reason to depart from Hutton's position as to the recurring cycle of events in the history of continental land? I think it must be admitted that the isostatic balance was far more frequently disturbed in what we may call Lower pre-Cambrian times than it has been in more recent periods. Osmond Fisher [16] has pointed out the possibility of local melting of the substratum of the crust by convection-currents in a liquid layer, and the consequent weakening of the mass above. The differences in composition of various parts of the crust render them, moreover, susceptible to fusion in various degrees, whatever may be the source of the heat by which they are attacked. Local fusion must indeed be regarded as an important cause of crustal weakening. If we wish to study the nature of the process, it is reasonable to examine regions that have at one time lain deep within the crust. Such regions are provided by the broad surfaces of Archæan rocks that were worn down through continental decay before they sank beneath the Cambrian sea.

It is well recognised that an ancient continent, resembling in most of its features the present 'Russian platform,' at one time stretched across the northern hemisphere. Wherever later deposits have been stripped from its surface, from central Canada to the Urals, and probably far beyond, we find that the older materials of this undulating continental platform consist largely of intrusive igneous rocks. These, moreover, have frequently a gneissic structure. Again and again, strongly banded gneisses occur, in which granitic material, verging on aplite, alternates with sheets of hornblendic or biotitic schist. The biotitic varieties can often be traced back into amphibolites. In places, lumps of these amphibolites are seen, streaked out at their margins, and providing a clear explanation of the dark bands throughout the gneiss.[17] This swallowing up of a mantle of basic material by a very different and highly siliceous magma rising from below is, indeed, seen to be a world-wide feature, wherever we find the lower crust-layers brought up within reach of observation. The tuffs and lavas of the Keewatin series have supplied the dark material in Canada, and similar rocks have been worked up into the gneisses of Galway, Stockholm, and Helsinki. The frequency of amphibolite in these ancient composite rocks is explained by the fact that this type of rock is the final term of various metamorphic series. While many lumps, for instance, in the gneisses of Donegal are residues of Dalriadian dolerites (epidiorites), others, rich in garnet and green pyroxene, and often containing quartz, are derived from a mixture of sediments in which limestone has been prevalent.[18] During the absorption and disappearance of these masses in the invading granite magma, the

[16] *Physics of the Earth's Crust*, ed. 2 (1889), p. 77.

[17] Since the historic works of A. C. Lawson (for example, 'Report on Rainy Lake Region,' *Geol. Surv. Canada*, Ann. Report for 1887, plates v. and vi.), these features have been traced in many areas. Compare W. H. Collins, 'Country between Lake Nipigon and Clay Lake, Ontario,' *Geol. Surv. Canada*, Publication 1059 (1909), p. 52; A. L. Hall, Presidential Address on the Bushveld Complex, *Proc. Geol. Soc. S. Africa*, 1914, p. xxii.; P. A. Wagner on Rhodesian gneisses, *Trans. ibid.*, vol. xvii. p. 39; and works cited in the next reference.

[18] See Michel Lévy, 'Granite de Flamanville,' *Bull. carte géol. France*, vol. v. (1893), p. 337; G. A. J. Cole, 'Metamorphic Rocks in E. Tyrone and S. Donegal,' *Trans. R. Irish Acad.*, vol. xxxi. (1900), p. 460; O. Trüstedt, 'Die Erzlagerstätten von Pitkäranta,' *Bull. Comm. géol. Finlande*, No. 19 (1907), pp. 72 and 92; F. D. Adams and A. E. Barlow, *op. cit.* (1910), pp. 25 and 97; F. Kretschmer, 'Kalksilikatfelse in der Umgebung von Mährisch-Schönberg,' *Jahrb. k. k. geol. Reichsanstalt*, vol. lviii. (1908), p. 568; &c., &c.

amphibole acquires potassium and breaks down into biotite, and biotite-gneisses result, which may extend over hundreds of square miles.

The details of such an igneous invasion are worthy of careful study, since only in this way can we follow out the progress of sub-crustal fusion. We see the highly metamorphosed material further attacked by the great cauldrons under it, and becoming seamed with intersecting veins. Block after block has been caught, as it were, in the act of foundering into the depths. In the gradual absorption of these blocks, and their penetration by insidious streaks of granite, we see pictured on a few square yards of surface the destruction of a continental floor.

To realise the magnitude of the process, however, hand-specimens and museum-specimens will not suffice. Here, as in all branches of geology, travel is the best of teachers, and the finest illustrations in a Geological Survey memoir will not convey the same impression as one wave-swept island of the Finnish skägård, a glaciated hillside in Donegal, or a dome of the Laurentian peneplane, from which the forest has been burnt away.

I am aware that in this statement of the relations of the Archæan gneisses to the overlying floor I am neglecting the effects attributed to dynamometamorphism. A rise of temperature, leading to molecular readjustment, is usually admitted by those who lay stress on the evidence of pressure, and study in the field along what may be called regional contacts produces the impression that thermal influence is a very potent factor. This is not the place for a review of the position taken by French observers,[19] who have done so much to enlarge our ideas of contact-alteration and intermingling. It is sufficient to remark that such contact-alteration, acting over wide areas, including as it does the advance of permeating liquids, goes far to account for the 'mineralisation' of previously normal sediments, while the injection of which there is such abundant evidence will explain the numerous varieties of composite and banded gneiss. J. Lehmann [20] restricted the term 'gneiss' to foliated igneous rocks in which the parallel structure was due to original flow or subsequent crushing, and denied the existence of a passage from mica-schist to gneiss. He left no place in his classification for the composite rocks that furnish so much information as to the methods of sub-crustal fusion. When such an authority as J. P. Iddings [21] hesitates to accept either the assimilation or the stoping theory as explaining the advance of an igneous magma on a large scale in the crust, I feel that I may not be able to carry all my hearers with me in this part of my argument as to the causes of terrestrial collapse. I can only say that I support certain conclusions, because I can conceive no other reading of the evidence offered in the field. When it is asserted that the earth is not hot enough to allow of the melting of one rock by another, I can only reply that such melting has taken place. The influence of liquids and gases in promoting fusion has been emphasised by Iddings, Judd, and Doelter. Even with this aid, we may not be able to explain the facts, though I think that we have gone a long way towards doing so. In the history of all the sciences, however, observation has run far beyond understanding.

Fortunately the argument that I hope to develop as to the comparative rapidity and possibly catastrophic character of certain crustal changes depends only in part on views that are still under discussion.

The invasion of a 'hard and brittle' [22] crust by an attacking magma was finely described by Lawson in 1888. Lawson pointed out that the Laurentian

[19] Michel Lévy summarised his views, side by side with essays by other authors, in 'Études sur les Schistes cristallins,' *C. R., Congrès géol. internat.* 1888, p. 117. See also P. Termier, 'Les Schistes cristallins des Alpes occidentales,' *C. R., Congrès géol. internat.*, 1903, p. 571. Compare numerous later observers, such as P. S. Richarz, 'Umgebung von Aspang,' *Verhandl. k. k. Reichsanstalt*, 1911, p. 285.

[20] *Op. cit., C.R., Congrès géol. internat.*, 1888, p. 115.

[21] *Igneous Rocks*, vol. i. (1909), p. 282; *The Problem of Volcanism* (1914), p. 200.

[22] A. C. Lawson, *op. cit.* p. 140. See also his revision of the area, *Geol. Surv. Canada*, Memoir 40 (1913).

gneisses gave no evidence of having 'yielded to pressures and earth-stresses.' The folding of the overlying series was prior to the solidification of the gneisses, and occurred [23] 'while the latter were yet in the form of probably a thick, viscid magma upon which floated the slowly shrinking and crumpling strata of the Coutchiching and Keewatin series. . . . Large portions of these rocks have very probably been absorbed by fusion with the magma, for the Laurentian rocks appear to have resulted from the fusion not simply of the floor upon which the Coutchiching and Keewatin rock first rested, whatever such floor may have been, but, also, with it, of portions of those series.'

The intense crumpling of the lower portion of the invaded series is not correlated by Lawson with the approach of the invader. The conversion of the lowest Archæan series, the Coutchiching sediments, into crystalline schists is, however, attributed to thermal metamorphism, and to hot vapours streaming from the molten floor.[24] Lawson realised the importance of shattering in allowing a magma to advance into an overlying 'brittle' series, and he is, so far as I know, the first observer to develop in satisfying detail what is now known as the stoping theory of igneous intrusion. J. G. Goodchild[25] soon afterwards described a striking example of rock-destruction by stoping and assimilation in the west of Scotland, where one of the 'newer granites' enters the Moine schists, and J. J. Sederholm,[26] in dealing with 'Ådergneis' in Finland, extended Lawson's views in a new field of regional metamorphism.

James Hutton always had in mind the effect of heat in 'softening' lower layers of the crust. His consolidation of strata by heat is preceded by a stage of melting. Sederholm, while referring back to Hutton as the pioneer, shows how in the vein-gneiss stage the unmelted sediments exhibit plasticity and become intensely contorted. The softening, in fact, induces flow. There is here no crushing or mylonitisation, but rather a viscid running of constituents, some on the verge of fusion, some, I venture to think, actually fused. Such rapidly repeated and intricate folding in metamorphosed sediments has been described as 'shearing' by some authors. Neither in the field nor in thin sections under the microscope can such a position be sustained. Shearing or attempted shearing may subsequently produce what has been called 'strain-slip cleavage' in the folds; but the folding has an earlier origin, and is very often associated with thermal changes. It is most intense when *lit par lit* injection has set in, and when the whole composite mass has become weak and plastic. The presence of confined water in aiding this plasticity must on no account be overlooked.

It may be well to illustrate this contention by one or two concrete instances from districts not remote from us at the present time. The noble cliffs of Minaun in Achill Island have been worn by the Atlantic from a mass of evenly bedded quartzites of Dalriadian age. These are invaded by veins of a very coarse red granite, the main mass of which lies below the present sea-level.[27] The edges of the strata appear fairly horizontal on the cliff-face; but contortion sets in towards the base, and the hard resisting rock has here[28] 'undergone intense crumpling and overfolding, such as one meets with on a large scale in mountain ranges, and this contorted flow seems entirely due to the yielding that has taken place in the region of heating.' The veins have broken in sinuous forms across the folds, just as they do in the intensely contorted vein-gneisses of Finland. They here represent a late episode, occurring

[23] *Op. cit.* on Rainy Lake, p. 131.

[24] Compare P. Termier, 'Schistes cristallins des Alpes occidentales,' *C. R., Congrès géol. internat.*, 1903, p. 585.

[25] 'Note on a Granite Junction in the Ross of Mull,' *Geol. Mag.* 1892, p. 447. Compare T. O. Bosworth on same area, *Quart. Journ. Geol. Soc. London*, vol. lxvi. (1910), p. 376.

[26] 'Ueber eine archäische Sedimentformation im südwestlichen-Finland,' *Bull. Comm. géol. Finlande*, No. 6 (1899), p. 133; and 'Ueber ptygmatische Faltungen,' *Neues Jahrb. für Min.*, Beilage Band 36 (1913), p. 491.

[27] *Proc. Geol. Assoc.*, vol. xxiv. (1913), Plate 17.

[28] G. A. J. Cole, 'Illustrations of Composite Gneisses and Amphibolites in N.W. Ireland,' *C. R., Congrès géol. internat.*, Canada (1913), p. 312.

in place at the top of the granite mass and in time at the close of its advance. The large size of the constituent crystals of the granite indicate that the surrounding rock was still maintained at a high temperature.

The features induced in the quartzite resemble those that are due in other cases to regional deformation. It may be urged that they represent some such crumpling during the rise of a laccolitic dome. Though such a dome, in Gilbert's view, implies stretching rather than compression, pressure from one side may thrust the rising beds together and produce local overfolding. On the other hand, the short section exposed in Achill may be part of a synclinal down-sagging. It is difficult, however, in either case to avoid associating this limited field of disturbance with the proximity of the igneous rock, particularly when other cases can be compared with it. South of Foxford, for instance, in the county of Mayo, the granite of Slieve Gamph invades a series of mica-schists and quartzites. Further west, and over a wide area towards Castlebar, this granite has become darkened and gneissose through assimilation of the schists; but here above Kilmore its advance was stayed, and the margin is cut, as usual, by veins that filled the cracks both of the main granite and the metamorphosed sediments. These sediments have become, prior to the shattering, crumpled and overfolded along the contact-region, and the section upon the glaciated slope resembles that of a fluidal rhyolite on a highly magnified scale.

The wonderful contortion of the composite mass that forms the north end of the Ox Mountains (Slieve Gamph) in the county of Leitrim gives a similar impression of viscid flow. The melting of a single constituent of the invaded schists, which here include amphibolites, would enable them to yield in response to the pressures that were forcing the granite magma in thin sheets between them Their metamorphism is thermal, and the forces that have produced the crumplings are not those of shearing acting on a solid mass, but may have operated from a distance hydrostatically through the magma.

Again, where limestones occur near granite contacts amid a series of various sedimentary types, they display folded structures in an altogether exceptional degree. Silicates have developed along their bedding-planes, but these have become contorted and rolled upon one another as metamorphism reached its maximum stage. At Maam Cross and Oughterard in the county of Galway, along the margin of the great granite mass that stretches thence southward to the sea, these flow-structures are conspicuous on weathered surfaces. The remarkable structures, moreover, of the 'skarn' of Fennoscandia, though otherwise explained by Trüstedt[29], may possibly represent an extreme stage of thermal alteration acting upon ordinary limestone.

Sederholm[30] refers contortion on a large scale to the latent plasticity of the rocks in the deeper layers of the crust, that is, in the 'plastosphere,' where the pressure is sufficient to translate this plasticity into flow. His researches on the basement-levels of the crust suggest, moreover, that in the depths heating becomes a very important factor in aiding this plasticity.

The main object of the foregoing discussion is to point out that the Huttonian cycle, in which thermal changes play so large a part, implies a serious weakening of the crust as magmas advance into it from below. The extensive metamorphism of the pre-Cambrian strata, which amounts to a distinctive feature, must, I think, be attributed, not to special intensity of tangential pressures in early times, but to frequency of igneous attack. Much of the crumpling of our schists may result from Hutton's 'softening,' the pressure being supplied from superincumbent masses, or even hydrostatically, and the flow occurring laterally, or vertically downwards, towards regions where destruction by absorption was going on. The features seen during the falling in of the walls of the lava-lake of Kilauea in Hawaii afford some idea of what takes place in zones of melting within the crust.

Were the scenes familiar to travellers at Kilauea to occur on a continental scale, they would be regarded as part of the regular order of nature, but would

[29] O. Trüstedt, *op. cit.*, *Bull. Comm. géol. Finlande*, No. 19 (1907), p. 91.

[30] 'Weitere Mitteilungen über Bruchspalten,' *Bull. Comm. géol. Finlande*, No. 37 (1913), p. 66.

none the less appear catastrophic in their intensity. Were they subterranean, they would be felt at the surface in more or less degree, according to the depth of the melting zone. In early pre-Cambrian times, whether a cooling molten earth was resenting its first imprisonment in a crust, or whether the collision of meteorites or the concentration of radio-active matter towards the surface was beginning to make itself effective, the zone of melting appears to have influenced great thicknesses of overlying strata. This influence was not world-wide at one and the same epoch, as we may see by the preservation of slightly altered sediments in certain places; but it was vigorous, menacing, and recurrent.

Under such conditions, even the surface-rocks must have fallen in at some points and have been replaced by igneous extrusions. Isostatic adjustments must have been very frequently disturbed. Folding of rocks, as a phenomenon of lateral surge and flow, must have made itself freely felt at the earth's surface. It is safe to assert that such conditions have not been repeated on a broad scale at any geological period subsequent to the spread of the Olenellus-fauna. Geochemical evolution, however, may have surprises still in store, and, in spite of long tradition, we are disinclined nowadays to rely too strongly on arguments based upon the sanctity of human life.

Possible Breaks in the Slow Continuity of Earth-movement.

1. *The Mountain-building Stage.*

Even with the thickened sedimentary crust beneath us, and the confidence inspired by our limited experience of the earth, we may ask if subterranean changes may not still result in catastrophes at the surface. Volcanic paroxysms have been regarded complacently as safety-valves, and the destruction of thirty thousand persons in a few minutes in Martinique or the Straits of Sunda form interesting historic episodes, when viewed from the platform or the pulpit of survivors in other lands. The 'grand theatre of action,' as Lyell says, may shift; but we feel that it will not do so in our own time. Some people live under towers of Siloam, others in San Francisco; but, after all, the menace appears small to the teeming humanity of the earth. The rise of an imperial dynasty at our side in Europe is more to be dreaded than that of isogeotherms beneath us.

What, however, is likely to occur if a mountain-building episode again sets in? Such episodes, affecting very wide areas, have undoubtedly recurred in the earth's history. We do not know if they are rhythmic; we do not know if they represent a pulsation, decreasing in intensity, inherited from the stars and hampered by increasing friction; we do not know if they record internal chemical changes, which have no climax, because they are neither cyclic nor involutionary, but evolutionary. The mid-Huronian chains, now worn down and supplying such valuable horizontal sections, were evidently of great extent; but we cannot say that they were vaster than those of later times.

The phenomena accompanying the growth of a single chain in the Cainozoic era give us, at any rate, ample food for thought. Though the narrow cross-section of the core of such a chain limits our field of observation, the same impressings of igneous material, and the same features of rock-weakening and rock-destruction, may be observed as in the immense basal sections exposed in the Archæan platforms. The progress of geological time has not diminished the activity in the depths. The granodiorite of western Montana,[31] for instance, which intruded during an uplift in early Eocene times, has attacked the Algonkian sediments of the district, producing phenomena of stoping and assimilation in the true 'Laurentian' style.

In the western and central Alps, again, the absence of any fossiliferous strata older than the Carboniferous arouses some surprise, until we find that many of the granitic intrusions are of late Carboniferous age. The crystalline schists

[31] J. Barrell, 'Marysville district, Montana; a study of igneous intrusion and contact metamorphism,' *U.S. Geol. Surv.*, Prof. Paper 57 (1907), and W. H. Emmons and F. C. Calkins, "Phillipsburg Quadrangle," *ibid.*, Paper 78 (1913).

west of Časlav in Bohemia and in the Eisengebirge are now attributed by Hinterlechner and von John[32] to the metamorphism of Ordovician strata by younger granite, which intruded in post-Devonian and probably in Carboniferous times. Much of the gneiss and granite of the Black Forest and the Vosges is now, moreover, removed from the Archæan, and is shown to be associated with the Armorican movements.[33] These vast intrusive masses occupy the place of strata of pre-Permian age. The great development of thermal metamorphism in the Erzgebirge and in Saxony,[34] two classic regions of the dynamometamorphic school, is now widely recognised, and this activity is also assigned to late Carboniferous times. The work of C. Barrois in Brittany is concerned with absorption-phenomena resulting from intrusions during the same mountain-building epoch.

Sederholm[35] has suggested that the ground above an area affected by processes of mountain-building cracks and becomes faulted, while the more plastic zone below flows under pressure into folds. But the blocks of the 'brittle' layer, as Lawson has it, may be seriously displaced by movements in the zone of folding, and subsidences of a regional character may occur. The example of the Minaun Cliffs in Achill shows that the plastic zone may become locally thickened by softening and overfolding. The pressure that has driven an excess of matter to the region of overfolding has squeezed it from beneath an adjacent region. Crumpling and overfolding are accompanied by a shearing away of the matter in one zone from that of another which overlies it; this must result in considerable disturbance of the zone nearer the surface.

We usually regard such disturbances from the uniformitarian point of view. Earthquakes are often bad enough, but they are treated as breaks in a slow process of folding that is always going on beneath our feet. May not, however, actual mountain-building be the break in a slow process of 'softening,' to use Hutton's term? For a long time the isostatic balance suffers only small disturbances, restoring itself automatically on a gently-yielding underworld. Then something gives way; something—a large mass of supporting rock—suffers a change of state. The balance is destroyed abruptly, and mountain-building and rapid subsidences have their day. O. Ampferer,[36] with his customary largeness of view, has referred superficial evidences of disturbance, such as mountain-ranges, to dragging movements of a mobile *Untergrund*. He urges that physical and chemical changes within the earth may produce considerable local changes of volume. Vertical movements lead to upfolding, and this leads to gravitational sliding. The zone of folding that we have been considering as normal near the *Untergrund* thus becomes transferred to the surface of the earth.

I am not now concerned with the causes of folding, beyond the fact that at a certain critical stage the material involved may move at a rapid rate. When R. A. Daly speaks of an 'orogenic collapse,'[37] he implies something of a different order in time from the slow processes of sinking and accumulation of sediment that have gone before. Changes of state, physical and chemical, occur with some abruptness. In the case of rocks, the softening or melting of even one constituent may allow of flow, and, as we have observed, this flow in a lower layer may soon become manifested in surface-changes.

Ampferer and Hammer[38] have recently considered the question of collapse in an opposite sense to that of Daly, who pictures it as the herald of an upward movement. These authors, on the other hand, regard the overfolded structure of

[32] *Verhandl. k. k. Reichsanstalt*, 1910, p. 337, and *Jahrb. ibid.*, vol. lix. (1909), p. 127.

[33] P. Kessler, 'Die Entstehung von Schwarzwald und Vogesen,' *Jahresberichte Oberrhein. geol. Vereines*, vol. iv. (1914), p. 31.

[34] C. Gäbert, *Zeitschr. deutsch. geol. Gesell.*, vol. lix. (1907), p. 308; R. Lepsius, 'Geologie von Deutschland' (1910), Pt. 2, pp. 107 and 172.

[35] *Op. cit.*, *Bull. Comm. geol. Finlande*, No. 37, p. 66.

[36] 'Das Bewegungsbild der Faltengebirgen,' *Jahrb. k. k. geol. Reichsanstalt*, vol. lvi. (1906), p. 607.

[37] *Igneous Rocks and their Origin* (1914), p. 188.

[38] O. Ampferer and W. Hammer, 'Geologischer Querschnitt durch die Ostalpen,' *Jahrb. k. k. Reichsanstalt*, vol. lxi. (1911), p. 700.

mountains as due to a considerable local reduction in volume of the *Untergrund*. The upper crust presses inwards from opposite sides, and the parts that are thrust downwards become absorbed and carried away with the retreating region of the *Untergrund*. The surviving parts fall over on either side, producing, as the whole continues to close in, folds that are not so very different from the now familiar *nappes de recouvrement* which these authors hesitate to accept. It is not clear why the postulated reduction in volume in the substratum should occur along a certain line, so as to give rise to axial folding at the surface. The folded mass, broken up by overthrusts, as sketched by Ampferer and Hammer, suggests in its general outlines a Jerusalem artichoke rather than a breaking wave.

The important point for our present purpose is, however, the restatement of the results of gravitation on the flanks of an uprising chain. The chain is said [39] to result from 'overthrustings with occasional involutions, accompanied by the rolling over and pushing forward of blocks' ('Ueberschiebungen mit gelegentlichen Einrollungen, Walz- und Schubschollen'). So long as these movements occur in the depths, they may be retarded considerably by friction; but, when they produce mountain-bulging at the surface, freedom is given to the 'waltzing' masses, and gravitation comes into play on the unsupported strata that flank the dome or anticline.[40]

Ampferer and Hammer [41] point out incidentally that much of the covering of Flysch and Molasse strata was worn away from the Alps before the final folding which gave the chain its eminence in early Pliocene times. They argue that the uplifted masses were thus less imposing than those pictured by G. Steinmann or C. Schmidt in their diagrams of the *Decken* or recumbent folds. But the view advanced by Ampferer and Hammer allows the principal folding to take place in contact with the upper air. The cover is usually supposed to act as a restrainer, and the long duration of the folding movements is held to have allowed of contemporaneous denudation. Cases, for instance, are known to us where rivers have maintained their level while crust-blocks rose beneath them.[42] The surprising thing, however, about our folded mountain-chains is the way in which they have been eroded parallel to the strike of the overthrust sheets or overfolds. Apart from occasional detached 'klips,' the distal parts of these masses must have been at one time continuous with those proximal to the root-region. The forward movement could not have occurred if denudation had negatived the effects of folding on the surface. A. Tornquist [43] has suggested that Jurassic limestones were pushed in among unconsolidated mudbanks while the Eocene sea still lay across the Alpine area. Anything comparable with this during the final folding could not fail to produce a largely felt disturbance at the surface. It is impossible to believe that the ground does not part asunder in Sederholm's zone of fissuring during the exceptional movements that rear a folded mountain-chain. Where such a zone has remained with its parts in contact, as in Fennoscandia, mountain-building has not really taken place. Intense folding may have occurred in the plastic zone below; but no one of the lines of superficial weakness has been continued as a plane of fracture into the depths. When this continuation occurs, the material of the folded zone may be forced up to the surface. The general deep-seated crumpling then involves the beds over it and becomes concentrated as an axial chain.

The marine or lacustrine deposits of the age immediately preceding that of uplift obviously cannot be consolidated at the epoch of upheaval. Gotlandian sands and muds must have overlain the heaving masses that rose as Caledonian land. The swamps of the Coal Measures were contorted in the Armorican chains; the highest beds of these must have been as yielding and as capable of

[39] *Ibid.*, p. 701.

[40] Compare O. Ampferer, 'Das Bewegungsbild von Faltengebirgen,' *Jahrb. k. k. Reichsanstalt*, vol. lvi. (1906), p. 601.

[41] *Op. cit.* (ref. 38), p. 708.

[42] This is urged even for the Himalayas. Medlicott, Blanford, and Oldham, *Geology of India*, ed. 2 (1903), p. 463.

[43] 'Noch einmal die Allgäu-Vorarlberger Flyschzone und der submarine Einschub ihrer Klippenzone,' *Verhandl. k. k. Reichsanstalt*, 1908, p. 330.

flow as the Flysch that overlay the growing Alps. In all these cases, familiar to us in Europe, the covering masses must have responded to the crumpling under them, and, when reared to dangerous eminences, rapidly became a prey to denudation and gravitational downsliding. They can scarcely be regarded as protective, and their removal would leave the brittle masses below more liable to fracture and to the 'calving' process that forms klips.[44]

Whether or no we postulate a yielding cover, the folding or overthrusting thus becomes part of the phenomena on the surface of the globe. This fact is glossed over in some of the admirable diagrams and models that have been prepared for our instruction, where the Flysch, for instance, above the overfolds is left without structural lines. Yet I venture to think, as I have already written,[45] that in the Alps 'so much occurred within a single epoch, the Tortonian or Upper Miocene, and probably in a few thousand years, that some of the movements must have been visible to the eye of man, had so discerning a creature appeared upon the scene. Earthquake-shocks at the present day produce perceptible undulations of the ground, and may leave permanent traces in the form of faults and dislocations. But it seems doubtful if a succession of small movements such as man has been able to record represents anything like the building of a mountain-chain when the resistance of the rocks has been overcome. . . . Slow as the general movement may have been, the crumpling was not confined to the hidden layers of the crust. It occurred in the rocks that formed the very surface, and the final drop into the lowlands suggests the features of a landslide.'

This conception was natural to the minds of our predecessors before a salutary check was given to those who demanded frequent and world-wide 'revolutions.' But nothing since that time has altered our impressions of the vast forces latent in the earth, ready to perform work when unbalanced and set free. The pressure that produces the solid flow foreseen by G. P. Scrope,[46] and demonstrated in C. Lapworth's mylonites, is capable of rearing folds to dangerous elevations at the surface.

The danger lies in the form of the fold in relation to its base. Ampferer and Hammer[47] urge that the basal rocks are absorbed into the depths, since the folded strata, when spread out again, would cover a far larger area than the crystalline cores beneath them. The upper layers therefore tend all the more to slide and fold on one another. Gravitation alone becomes under such circumstances a potent cause of surface-crumpling.

G. P. Scrope[48] felt that the uprise of a chain was in itself sudden and paroxysmal. We may go so far—or, shall we say, go back so far?—as to realise that large shifts may be made suddenly along thrust-planes when crushing takes place and resistance has been overcome. We can feel greater confidence, however, when we consider the gravitational movements outwards from the line of upfolding. These may be either one-sided or two-sided. Scrope[49] represents the lower zone in a mountain-chain as flowing by pressure towards a line of weakness, and the upper zone of rocks as flowing by gravitation away from this line on both sides. He believed that, in addition to crumpling and the production of recumbent folds, actual fissuring might occur. He explained in this way the isolated blocks of the Dolomite Alps of Tyrol. 'Fracture chasms' may not have occurred between these particular blocks, though differential move-

[44] Something of this kind must have been pictured by C. L. Griesbach, when he wrote ('Exotic Blocks of the Himâlayas,' *C.R.*, *Congrès géol. internat.*, 1903, p. 551): 'Much of the older sedimentary rocks must have been brought to the surface, not only as part of the sections, but also in crushed masses and detached blocks torn off from situations *in situ*, a phenomenon common to all disturbed areas. The outcrops of dislocations which have later undergone weathering and denudation must, of course, have been shorn of all crushed and loose fragmentary masses.'

[45] *The Growth of Europe* (1914), p. 173.

[46] *Considerations on Volcanos* (1825), pp. 202 and 234.

[47] *Op. cit.* (ref. 38), p. 708.

[48] *Considerations on Volcanos* (1825), pp. 202 and 234.

[49] *Ibid.*, pp. 202 and 204.

ments—perhaps even 'waltzings'—along fault-planes have been proved; but is there anything in Scrope's position that is really more extreme than the klip-theory of the present day?

Klips must be regarded as blocks cut off from the main region of an overfolded or an overthrust mass. Denudation occurring after the forward movement suffices to explain some cases; in others, separation seems to have taken place as the moving mass fell forward. The klips of hard material embedded in softer strata are thus a kind of rock-spray, hurled in advance of the breaking earth-wave. C. Schmidt[50] in our own time pictures the transference by gravitation of strata from above the St. Gothard gneiss to the lowland of the Lake of Uri. This lowland has become enriched, then, from a scenic point of view at the expense of the unstable central range.

In times later than those of the fathers of geology, the apostle of gravitational movement as a cause of overfolding has undoubtedly been Edvard Reyer. He has recently restated his views on the origin of mountain-ranges,[51] and he must be satisfied with the general concurrence that sliding is a factor to be considered. He may be especially pleased with P. Termier's[52] description of the overriding of the Pelvoux mass by earth-waves from the east, where the isoclinal folding of the strata on the west side of the chain is referred to the passage of sheets across them, which have since disappeared through denudation. The folding, says Termier, in his admirable prose, records the outward movement of the sheets, just as the forms of the trees in the Provençal plain record the passage of the mistral. Termier in no wise fears to speak of the progress of a 'traîneur écraseur' during mountain-building as 'soudain et rapide comme une rupture d'equilibre, le dernier acte, longuement préparé, mais joué d'*emportement*, de ce drame grandiose.'

Rupture combined with rapid movement of the rocks need not be the last act of the drama; but, the more we examine the history of folded chains, the more probable it appears as a culminating episode. The infolding and infaulting of strata, such as the Siwalik Beds in India, at the base of a rising chain may be a matter of slow squeezing. We see in India how denudation has been at work during the overthrusting process; but the successive movements may none the less have included rapid phases. When a fan arises in a geanticlinal by nipping at the base, its destruction also may be rapid, since it is assisted by rupture and falling apart of the upper portions of the folds. The original cover of our present ranges has been lost by denudation. Earth-sculpture in these regions of high altitude and vehement attack has removed much of the evidence that we seek. What remains, however, may lead us to feel that no part of the world in historic times has experienced a mountain-building episode.

Such relatively catastrophic stages have, indeed, not been common in the long history of the earth since pre-Cambrian times. It appears that now and again the 'orogenic collapse' of some considerable area disturbs the balance in the crust and spreads far through the upper layers like a disease. Or it may be that the thermal cause of the collapse is common to the whole earth at the same time, and becomes manifest in responsive regions far apart. In any case, the weak places give way and the more resisting ones close in. A re-adjustment is effected, which then endures through long geological time.

Radio-active measurements of the length of geological periods may some day enable us to determine if the major disturbances of the crust are rhythmic. Present results, however, do not indicate a time-relation. The figures provided by W. J. Sollas,[53] and based on the thicknesses of strata, give us the following intervals between some of the best marked foldings of the crust. The unit here employed is the time represented by 1,000 feet of strata. The formation of the Huronian chains must be set back by an unknown amount into the pre-Cambrian era, since the crumpled masses, invaded by the younger series of

[50] *Bild und Bau der Schweizer Alpen* (1907), p. 68.

[51] *Geologische Prinzipienfragen* (1907), pp. 142, 147, &c.

[52] 'Les Problèmes de la Géologie tectonique dans le Méditerranée occidentale,' *Revue générale des Sciences*, March 30, 1911.

[53] Presidential Address, *Quart. Journ. Geol. Soc. London*, vol. lxv. (1909), *Proceedings*, p. 112.

'Laurentian' granites and gneisses, were greatly denuded before the lowest Upper Huronian (Animikian) strata were laid down.[54] This renders the first time-interval here given of very little value, and its resemblance to the third interval seems a matter of mere coincidence.

Interval from Middle Huronian to Caledonian (Lower Devonian) folding, (?) 122 units.
From Caledonian to Armorican (latest Carboniferous) folding, 51 units.
From Armorican to Alpine (earliest Pliocene) folding, 127 units.

Our search for a rhythm is complicated by the occurrence of more localised movements of considerable intensity at intervening geological epochs. The Caucasus, for instance, became strongly folded towards the close of the Jurassic period, when western Europe responded by a gentler groundswell. The Cretaceous beds on the flanks of the Harz Mountains were upturned in early Eocene times, when our English Chalk also came within reach of denudation. Heralds of this unrest may perhaps be seen in the vast outpourings of lava in central India towards the end of Cretaceous times; and it is clear that we must take into account such igneous upwellings, and also the occurrence of down-sinkings of large areas, when drawing up a history of energy within the earth.

As already observed, this energy seems effective enough at all epochs. The Armorican folding was accompanied by immense upwellings of molten matter from the depths, and the features of crust-weakening and absorption seem to have rivalled those that we can study in the basal sections of Archæan mountains. The Alpine movements were probably associated with equally intense igneous action, the extent of which will not be revealed until the present destructive phase of the Huttonian cycle is complete.

The imminent menace of crustal changes was brought home to us during the terrible period from April 4, 1905, to January 14, 1907, the final twelve months being marked by a veritable earth-storm.[55] Three years had elapsed since the catastrophic events of Saint Vincent, Martinique, and Santa Maria of Guatemala. On April 4, 1905, 20,000 persons perished through an earthquake in the Kangra Valley, on the flanks of the central Himalayas. On September 8 destruction was carried through Calabria. On January 31, 1906, the Colombian coast suffered from sea-waves flung upon it by the Pacific floor, and in March and April the unrest was manifested on the other side of the basin in Formosa. From the 4th to the 12th of April, Naples was endangered by one of the most serious eruptions of Vesuvius, which reduced the mountain by 500 feet and formed the present crater of explosion. On April 18 San Francisco was wrecked in sixty-five seconds, and the fires that broke out in the shattered city completed its destruction in five days. On August 16 Valparaiso and Santiago similarly suffered, and sea-waves signalled the earth-movement seven thousand miles away on the margin of Pacific isles. On January 14, 1907, the year of earth-storm closed with the ruin of Kingston in Jamaica.

The last great storm of mountain-folding, that which reared the Cainozoic ranges and marked out the edges of Eurasia and America, seems still to produce symptoms of unrest. Geologically speaking, however, we are near enough to the Tortonian epoch to look forward with some confidence to a quiescent phase. But some day, in its due season, the earth will once more be active. When that time comes, no ingenuity of man will suffice to meet it. Earthquake after earthquake, increasing in intensity, will probably have driven the population to a distance from the threatened zone. Concentration of the folding along a particular earth-line will limit the region of absolute destruction; but the undulations spreading from it, in response to the heavings of the chain, will offer sufficient chances of catastrophe. In the case of our youngest mountain-

[54] A. P. Coleman, Presidential Address to Section C, *Rep. Brit. Assoc.*, 1910, p. 598.

[55] The details are summarised in G. A. J. Cole, *The Changeful Earth* (1911), pp. 195-203.

ranges, these undulations remain perpetuated as domes and dimplings of the crust, which are already worn down or infilled respectively by denudation and deposition. Their present forms and places record the last movements of the earth-storm, just as a buckled tramway-rail records the passage of an earthquake. How shall we gauge to-day the intensity of their rise and fall?

In the case of the city devastated by an earthquake, the débris is cleared away, and our descendants in time discover the distorted rails beneath the healing mantle of new grass. Will they realise from this alone the preliminary tremors, the sudden arrival of the culminating vibration, the shock that overcame the elasticity of the crust beneath them, and then the gradual establishment of the conditions under which they have passed their peaceful lives? The crumpled wreckage lies there in evidence before them; but how will they distinguish the work of a few stormy seconds from that due to the gentle earth-creep of a century?

Possible Breaks in the Slow Continuity of Earth-movement.

2. Regions of Subsidence.

It was probably E. Suess who brought home to most of us the importance of regions of subsidence in defining the lowlands and the sea-basins from the upstanding masses of the crust. While one region may be folded, another may be broken into blocks; and the two types of movement, that due to tangential thrusting and that due to vertical uplift and down-faulting, may appear in the same region and may alike play their part in producing a lowering of large areas. The domes and dimples that occur beyond the region of acute crumpling may be intensified into fault-blocks by fracture of their boundaries. W. Salomon [56] has, moreover, shown us how the movements in the Rhine-trough, a typical region of block-foundering, may be linked with those associated with over-thrusting. The rise of one region is associated with the lowering of another. If catastrophes are possible during uplift, we may look for them also during subsidence.

The occurrence of lines of volcanoes in and along the edges of subsiding areas may be regarded as evidence of the squeezing of a previously fused substratum, or as evidence of sub-crustal melting which has primarily caused the subsidence. In either case, it is difficult to believe that the cracks along which the cones become established are formed gently and without elements of surprise. The dykes and cones represent a healing process. The accumulation of lava-sheets and the outpouring of ash from localised centres obscure what has gone on before. The uplift of the Pacific coast has included rapid stages, as C. Darwin recognised,[57] and as we have recently realised in Alaska. It is improbable that the downward movements of the sea-floor adjacent to it have been of a minor order, or that the larger movements of elevation have not been accompanied by similar movements of collapse.

The cutting-up of mountain-chains by transverse fractures has resulted in the loss of huge blocks beneath the sea. In such cases it is clear that faulting has run a long way ahead of denudation. The breaking of the chain that united Andalusia and the western Alps, the falling in of the Tyrrhenian earth-block, the subsidences at two separate epochs of the southern and northern Adriatic, and the conversion of the hilly mass between the Balkans and Asia Minor into the Ægean, set with islands, suggest a return to the girdling waters of the Tethys. The spread of the Atlantic northward, by fracture across the tough blocks of Armorican land, has led Pierre Termier to revive the story of Atlantis [58] as an episode of human times. Botanists and zoologists require a recent Atlantic land-surface as a field for migration and a refuge during Glacial cold. Who has recorded, except the Egyptian priest of Saïs, the precise mode of its disappearance beneath the waves?

All trough-valleys, which are often called, somewhat misleadingly, rift-valleys, raise the same questions as to the nature of the steps by which they have been produced. The Rhine Vale, one of the most closely studied examples,

[56] 'Die Bedeutung der Messen von Harnischen,' *Zeitschr. deutsch geol. Gesell.*, vol. lxiii. (1911), p. 515.

[57] *Geol. Observations on S. America*, Minerva edition, p. 293.

[58] 'L'Atlantide,' *Bull. Institut océanographique*, No. 256 (1913).

dropped 8,000 feet within the limits of Oligocene time. It is improbable that the numerous faults now traceable operated with concerted gentleness, or invited the Oligocene sea to lap in by imperceptible gradations from the west.

Abruptness of certain Geographical Changes.—River-capture.

There is a totally different class of terrestrial phenomena which lends itself also to speculation, or to that imaginative faculty, proper to our Section, which enables the geologist to reconstruct. The deluge that appeared to affect the world, as known to Chaldæan sages, has long been regarded as confined to a limited valley of western Asia. But geographers have taught us to speak lightly of river-diversion and river-capture, and to treat them as frequent occurrences in the history of existing lands. It is interesting to inquire what this process on a large scale may involve.

The draining of the Ragunda lake in Sweden[59] in 1796, by the rapid cutting of a ravine 100 feet deep in a soft barrier, shows how many of our Glacial lakes, dammed by morainic matter, may have excavated their outlet gorges and run dry in the course of a few hours. The history of the temporary lake behind the Gohna landslip, so brilliantly studied by our Vice-President, Sir Thomas Holland,[60] provided a lesson both in hill-destruction and catastrophic flooding. The diversion of the Colorado River, however, in 1905, into the sluice leading to the Salton Sink gives us a definite illustration of river-capture. The 'New River' thus produced in the depression to the north-west of Calexico cut a valley seventy feet deep through the agricultural land that it was meant to serve, and worked the head of this valley backward at the rate of a third of a mile a day.

One of the most remarkable instances of river-diversion in the European record is that of the waters from the north side of the central Alps. At the close of the Pliocene period, the chain had already become grooved by the subsequent valley of the Rhone, and this river had been shifted, by earth-movement in the Juras, south-westward towards its present course at Geneva. The north slopes of the St. Gothard mass and the Bernese Alps, supplying the torrents of the Reuss-Aar-Saane system, still, however, drained across the hummocky land near Bâle and sent their waters over to the Doubs. The great Rhine-trough drained southward, and its streams formed tributaries of the Alpine flow near Bâle.

The Mainz basin, however, which was infilled by Lower Pliocene alluvium, became tapped by the head of a river that had long run northward from the Hunsrück-Taunus range. This river is the Rhine that we know north of Coblenz, and its alluvium was then spread out where the sea now stretches between Holland and the Yorkshire coast. Its mature valley is still traceable[61] above the present stream-cut in the hills. This river could have no direct influence on the course of the drainage from the Alps. But the bulging of the land at the north end of the Juras still continued. As the text-books remark with some complacency, the Burgundian gate was closed, and the river that had previously crossed westward was diverted northward to the Rhine-trough.

Can we exactly picture what this means? The whole Reuss-Aar-Saane system 'on some particular day began to flow northward along the far older tectonic trough, carving away the infilling of detritus, washing back tree stems that were floating quietly from the Lake of Mainz on their way to the Mediterranean, and finding, when it reached that lake, a notch sufficiently low for its escape across the Hunsruck-Taunus range. An enormous body of water was thus added to that which had formed in Pliocene times a mature valley across these hills.'[62] The addition of the drainage of Graubünden, including the

[59] See especially H. W. Ahlmann, 'Ragundasjöns Geomorfologi,' *Sveriges Geol. Undersök*, 1915; also Ahlmann, Carlzon, and Sandegren, 'Quaternary History of the Ragunda Region, Jämtland,' *Geol. Fören. Förhandl.*, vol. xxxiv. (1912), p. 343.

[60] *Records Geol. Surv. India*, vol. xxvii. (1894), p. 55, and *Nature*, vol. l., p. 501.

[61] W. M. Davis, *Die erklärende Beschreibung der Landformen* (1912), p. 106.

[62] G. A. J. Cole, *The Growth of Europe* (1914), p. 109.

Vorder and Hinter Rhein and the Lake of Constance, to the water flowing through the trough-valley was probably an accident that occurred later than the Riss-age of the Glacial epoch. The system indicated above, representing the flow from a hundred miles of snow-clad mountains, must, however, have made a remarkable change in the stream across the Armorican hills. In time, as it lowered the loose deposits of the Mainz basin, this river carved out the young ravine that runs like a knife-cut through the range; the water that flows past Mainz, with the exception, perhaps, of that from Constance, represents the magnitude of the event that we speak of as the diversion of a stream. The abrupt change was not confined to the hill-region. When the Alpine water arrived at the Mainz basin, and found its way into the notch formed by the Pliocene Rhine, carrying with it mud from the glaciers of the Jungfrau and coarser alluvium from the old trough-valley, it poured down upon the forest-covered delta-land.

The changes that have occurred in the unconsolidated ground of Holland in historic times, including the loss of the Biesbosch, with its seventy-two villages, in a single night, furnish some picture of what must have happened in the prehistoric delta of the Rhine. Land was suddenly built up at some points, islands were carved out at others, and the effects of the catastrophe must have been still manifest when the Scandinavian ice-sheet began to invade the mud-flats from the north.

The capture of a large river may be illustrated by the story of the Vistula. This noble stream, the Rhine of Polish lands, represents in a remarkable way the drainage of 190 miles of the Carpathians. All this water becomes concentrated, at the apex of a reversed river-fan, at the east end of the Kielce hills, and it is probable that the upper Vistula was driven to join the San by the advancing ice-front of the Riss-age, and that both rivers then escaped southwards. The joint waters were again held up when the Fennoscandian ice rested along the line marked by the Baltic Heights, and it is well known that a great river flowed westward along the stagnating ice-front where now the marshes of the Netze mark its course. This river stretched away west to join the Elbe, and the water from the Galician highlands thus met that from Switzerland in the Anglo-Danish delta-land. As the ice-front shrank backward, towards the Baltic basin,[63] streams flowed down over the sands and boulder-clays and cut their valley-heads back southward. Overflows may have taken place on the unconsolidated wall of the great east-and-west river, which was now deprived of its barrier of moraine-filled ice. In one way or another, the shallow valley of the main river was tapped near Kustrin, and the Oder, rising in the Moravian plateau, was sent northward as an independent stream. Similarly, the Vistula was carried off at Fordon, where the bend due to capture is conspicuous at the present day; and the whole drainage of the north wall of the Carpathians swept across the drift deposits down the course of some hitherto unimportant stream. Along the valley thus carved out, brown and yellow cliffs now rise above the marshy flood-plain, and the red castles of advancing Germany have for centuries looked down firmly on the stream. It is quite contrary to our customary philosophy, but a good corrective all the same, to ask ourselves if this lower valley of the Vistula, eighty miles in length, was shaped in a few months or a few years. The main part of the excavation, across unconsolidated lands, may have occupied less time than the building of the strongholds at the fords.

Conclusion.

In spite of the swamping of the Alkmaar country in 1825, in spite of the tragedy of Messina only seven short years ago, we feel that Europe is a settled continent, and we judge the past and future by the present superficial peace. We have applied the same thoughts to human movements, and the inconceivable has happened in our midst. We naturally find it difficult to carry our minds back to epochs when the earth-blocks may have parted asunder as ice parts across the polar seas. We have, however, still very much to learn about causes now in action; and the mystery of the earth, and of our connexion with it,

[63] R. Lepsius (*Geologie von Deutschland*, Pt. 2, p. 511) urges that the sinking of the floor of northern Europe led to this northward trend of the streams.

grows upon us as we learn. Can we at all realise the greatest change that ever came upon the globe, the moment when living matter appeared upon its surface, perhaps over a few square miles? Matter is either dead or living, that is, endowed with life; there is no intermediate state. And here was living matter, a product of the slime, if you will, but of a slime more glorious than the stars. Was this thing, life, a surface-concentration, a specialisation, of something that had previously permeated all matter, but had remained powerless because it was infinitely diffuse? Here you will perceive that the mere geologist is very much beyond his depth. Let us return to our orderly studies, our patient hammerings, our rock-slices, our chiselling out of fossil shells. Behind it all is the earth itself, quiescent, it may be, but by no means in the sleep of death. As Termier puts it, 'La planète n'est pas encore morte; elle ne fait que dormir.' If in this Address I have dwelt upon the possibility of rapid changes in its surface, no member of our Association will feel the least alarm.

Felix qui potuit rerum cognoscere causas,
Atque metus omnes, et inexorabile fatum,
Subiecit pedibus, strepitumque Acherontis auari.

British Association for the Advancement of Science.

SECTION D: MANCHESTER, 1915.

ADDRESS

TO THE

ZOOLOGICAL SECTION

BY

PROFESSOR E. A. MINCHIN, M.A., HON. PH.D., F.R.S.,

PRESIDENT OF THE SECTION.

The Evolution of the Cell.

WHEN addressing an audience of biologists it would be superfluous to insist upon the importance of the study of the cell and its activities. It is now recognised almost universally that the minute corpuscles known by the somewhat unsuitable term 'cells' are the vital units of which the bodies of animals and plants are built up, and that all distinctive vital processes—metabolism, growth and reproduction, sexual phenomena and heredity—reduce themselves ultimately to activities taking place in, and carried on by, the individual cells which build up the body as a whole. Each cell must be regarded as a living, individual organism which, however much it may be specialised for some particular function or form of vital activity, is capable of maintaining its life and existence in a suitable environment by carrying on all the necessary processes of metabolism which are the essential and distinctive characteristics of living beings. In the case of cells composing the complex body of the higher animals and plants the cells are mutually interdependent, and, with the exception of the mature germ-cells, cannot maintain their existence apart from their fellows; that is to say, the only natural [1] environment suitable for their continued existence is the complex body or cell-commonwealth of which they form an integral part. But in the simplest forms of life the whole body of the living individual may reach no higher degree of complexity than the single cell, which is then seen as an organism physiologically complete in every respect, living a free and independent life in Nature and competing with other organisms of all kinds, simple or complex, in the universal struggle for existence amongst living beings. This statement of the 'cell-theory' is that with which, I believe, the majority of modern biologists would agree; not without, however, some dissentients, amongst whom I personally am not to be numbered.[2]

The fundamental importance of the cell as a complete living organism, whether maintaining itself singly and independently or in union with other similar but individually specialised units, has made it the object of intensive and concentrated study, not only by those who group themselves according to their special points of view as zoologists, botanists, physiologists, &c., but also by a class of investigators who take the cell itself as the subject of a branch

[1] It is not necessary to do more than refer here to the investigations that have been carried on in recent years with regard to the viability and multiplication of tissue-cells removed from the body in artificial culture-media. These experiments afford strong support to the view that the cell is to be regarded primarily as an independent living organism.

[2] See Appendix A.

of biological investigation termed cytology, which deals with cells in a general manner independently of their provenance, whether animal or vegetable. Some knowledge of the cell and its activities is necessary at the present time for every one concerned with the study of living things, whether that study is pursued for its own sake and with disinterested objects, or with the intention of applying scientific principles to practical aims, as in medicine or agriculture. One might have expected, therefore, that at least some elementary understanding of the nature and significance of the cell, and the importance of cellular activities in the study of life and living things, would have formed at the present time an indispensable part of the stock of knowledge acquired by all intelligent persons who are ranked as 'educated' in popular estimation. Unfortunately this is so far from being the case that it is practically impossible, in this country at least, to find anyone amongst the educated classes to whom the words 'cell' and 'cytology' convey any meaning at all, except amongst those who have interested themselves specially in some branch of biology. Consequently, any discussion concerning the cell, although it may deal with the most elementary processes of life and the fundamental activities and peculiarities of living beings, ranks in popular estimation as dealing with some abstruse and recondite subject quite remote from ordinary life and of interest only to biological specialists. It must, however, be pointed out that the general state of ignorance concerning these matters is doubtless in great part due to the fact that an objective acquaintance with cells cannot be obtained without the use of expensive and delicate optical instruments.

I propose in this address to deal with an aspect of cytology which appears to me not to have received as yet the attention which it deserves, namely, the evolution of the cell itself and of its complex organisation as revealed by the investigation of cytologists. Up to the present time the labours of professed cytologists have been directed almost entirely towards the study of the cell in its most perfect form as it occurs in the Metazoa and the higher plants. Many cytologists appear indeed to regard the cell, as they know it in the Metazoa and Metaphyta, as the beginning of all things, the primordial unit in the evolution of living beings.[3] For my part I would as soon postulate the special creation of man as believe that the Metazoan cell, with its elaborate organisation and its extraordinarily perfected method of nuclear division by karyokinesis, represents the starting-point of the evolution of life. So long, however, as the attention of cytologists is confined to the study of the cells building up the bodies of the higher animals and plants, they are not brought face to face with the stages of evolution of the cell, but are confronted only with the cell as a finished and perfected product of evolution, that is to say, with cells which, although they may show infinite variation in subordinate points of structure and activity, are nevertheless so fundamentally of one type that their plan of structure and mode of reproduction by division can be described in general terms once and for all in the first chapter of a biological text-book or in the opening lecture of a course of elementary biology.

One of the most striking features of the general trend of biological investigation during the last two decades has been the attention paid to the Protista, that vast assemblage of living beings invisible, with few exceptions, to the unassisted human vision and in some cases minute beyond the range of the most powerful microscopes of to-day. The study of the Protista has received in recent years a great stimulus from the discovery of the importance of some of the parasitic forms as invaders of the bodies of men and animals and causers of diseases often of a deadly nature; it has, however, yielded at the same time results of the utmost importance for general scientific knowledge and theory. The morphological characteristic of the Protista, speaking generally, is that the body of the individual does not attain to a higher degree of

[3] For example, my friend Dr. C. E. Walker, in an article in *Science Progress* (vol. vii. p. 639), after stating that 'The unit of living matter, so far as we know, is the cell,' proceeds to deal with 'that form in which it is found in the multicellular and the majority of unicellular organisms, both animal and vegetable' and then describes the typical cell of the cytologist, with nucleus, cytoplasm, centrosome, chrondriosomes, and reproduction with fully developed karyokinesis.

organisation than that of the single cell. The exploitation, if I may use the term, of the Protista, though still in its initial stages, has already shown that it is amongst these organisms that we have to seek for the forms which indicate the evolution of the cell, both those lines of descent which lead on to the cell as seen in the Metazoa and Metaphyta, as well as other lines leading in directions altogether divergent from the typical cell of the text-book. We find in the Protista every possible condition of structural differentiation and elaboration, from cells as highly organised as those of Metazoa or even, in some cases, much more so, back to types of structure to which the term cell can only be applied by stretching its meaning to the breaking-point. Already one generalisation of cytologists has been torpedoed by the study of the Protista. The dictum 'Omnis nucleus e nucleo' is perfectly valid as long as it is restricted to the cells of Metazoa and Metaphyta, to the material, that is to say, to which the professed cytologist usually confines his observations.[4] But in the Protista it is now well established that nuclei can arise *de novo*, not from pre-existing nuclei but from the extranuclear chromatin for which Hertwig first coined the term 'chromidia.'

It is clear, therefore, that the results already gained from the study of the Protista have brought about a new situation which must be faced frankly and boldly. It is impossible any longer to regard the cell as seen in the Metazoa and as defined in the text-books as the starting-point of organic evolution. It must be recognised that this type of cell has a long history of evolution behind it, which must be traced out, so far as the data permit. The construction of phylogenies and evolutionary series is of course purely speculative, since these theories relate to events which have taken place in a remote past, and which can only be inferred dimly and vaguely from such fragments of wreckage as are to be found stranded on the sands of the time in which we live. Many important stages of evolution may be totally submerged and no longer available for study and consideration. The extent to which such speculations will carry conviction to a reasonable mind will depend entirely on the stores of data that can be collected and which must be the last appeal for the cogency of all arguments and judgments. The study of the Protista is as yet in its infancy; groups have been recognised and have received ponderous designations although their very existence is yet in doubt, as in the case of the so-called Chlamydozoa; and our knowledge of the affinities and mutual relationships of the groups is still very imperfect. All attempts, therefore, to trace the evolution of the Protista must be considered as purely tentative at present. If I venture upon any such attempt, it is to be regarded as indicating a firm belief on my part that the evolution of the cell has taken place amongst the Protista, and that its stages can be traced there, rather than as a dogmatic statement that the evolution has taken place in just the manner which seems to me most probable. When we reflect on the irreconcilable differences of opinion amongst zoologists with regard to the origin and ancestry of vertebrates, for example, we may well be cautious in accepting pedigrees in Protista.

Before, however, I can proceed to deal with my main subject, it is absolutely necessary that I should define clearly the sense in which I propose to use certain terms, more especially the words 'cell,' 'nucleus,' 'chromatin,' 'protoplasm,' and 'cytoplasm.' Unless I do so my position is certain to be misunderstood, as, indeed, it has been already by some of my critics.

The term cell was applied originally by botanists to the single chambers or units of the honeycombed structure seen in the tissues of plants. The application of the term to such structures is perfectly natural and intelligible, since each such cell in its typical form is actually a closed space limited by firm

[4] Vejdovský (*Zum Problem der Vererbungsträger*, Prag, 1911-1912, p. 120) has already maintained, for the cells of Metazoa, that Fleming's aphorism 'Omnis nucleus e nucleo' should be changed to 'Omnis nucleus e chromosomatis' [*sic*], on the ground that the nucleus, as such, is not an original cell-component 'but is produced secondarily from the chromosomes of the mother-cell.' If this is true, there is but little difference in detail, and none in principle, between the formation of 'secondary' nuclei from chromidia and the reconstruction of a daughter-nucleus from chromosomes in the most perfected form of karyokinesis.

walls, and containing a relatively large quantity of fluid cell-sap and a small quantity of the slimy protoplasmic substance. When these structures were first discovered, the limiting membrane or wall of the cell was regarded as essential, and less importance was attached to its contents. With increased knowledge, however, and especially when animal tissues came to be studied, it became apparent that the cell-wall, like the fluid cell-sap, was a secondary product, and that the essential and primary part of the cell was the viscid protoplasmic substance, in which a peculiar body, the 'nucleus' or kernel, was found to be universally present. Consequently the application and meaning of the term cell had to undergo an entire change, and it was defined as a small mass or corpuscle of the living substance, protoplasm, containing at least one nucleus. To these essential constituents other structures, such as a limiting membrane or cell-wall, and internal spaces—vacuoles—filled with watery fluid, might be added as products of the secretory or formative activity of the living substance; but such structures were no longer regarded as essential to the definition of the cell, since in many cases they are not present. It is to be regretted in some respects that with this changed point of view the term 'cell,' used originally under a misapprehension, was not replaced by some other term of which the ordinary significance would have been more applicable to the body denoted by it.[5]

The chief point that I wish to establish, however, is that the term cell was applied originally to the protoplasmic corpuscles building up the bodies of the Metazoa and Metaphyta, each such corpuscle consisting of a minute individualised mass of the living substance and containing a nucleus. Hence a complete cell is made up of two principal parts or regions, the nucleus and the remainder of the protoplasmic body, termed the cytoplasm. By some authors the term protoplasm is restricted to the cytoplasmic portion of the cell, and protoplasm is then contrasted with nucleus; but it is more convenient to consider the whole cell as composed of protoplasm divided into two regions, nucleus and cytoplasm.

We come now to the consideration of the body termed the nucleus, which undoubtedly possesses an importance in the life and functions of the cell far greater than would be inferred from the name given to it. A nucleus, as seen in its typical form, has a limiting membrane enclosing a framework composed of a substance termed 'linin.' The framework has the form of a network, which is probably to be interpreted, primitively at least, as the optical expression of an alveolar structure similar to that seen also in the cytoplasm, but of coarser texture, and the apparent 'threads' of the linin-framework may then be the optical sections of the partitions between neighbouring alveoli. Such an interpretation does not exclude the possibility of the formation of real threads or fibres in the framework in certain cases or during particular periods of nuclear activity; just as fibrous structures may arise in the alveolar cytoplasm also. The cavities of the framework contain a watery fluid or nuclear sap, probably of the same nature as the fluid enchylema or cell-sap contained in the alveolar framework of the cytoplasm. At the nodes of the alveolar framework are lodged grains or masses of *chromatin*, a substance which must engage our most particular attention, since it is the essential constituent of the nucleus, universally present in all nuclei, whether of the simplest or of the most complex types. In addition to the chromatin-grains, which are distributed in various ways over the linin-framework, there are to be found usually one or more masses termed nucleoli, composed of a material which differs from chromatin in its reactions and has been termed plastin.

In the foregoing paragraph I have described in general terms the typical nucleus of the text-books, as found commonly in the cells that build up the bodies of ordinary animals and plants. The minutiæ of the details of structure and arrangement of the constituent parts may vary infinitely, but the type remains fairly constant. When we come, however, to the nuclei of the Protista, such pronounced modifications and variations of the type are met with

[5] 'Nothing could be less appropriate than to call such a body a "cell"; yet the word has become so firmly established that every effort to replace it by a better has failed, and it probably must be accepted as part of the established nomenclature of science.'—E. B. Wilson, *The Cell*, p. 19.

that a description in general terms is no longer possible. I shall deal with some of these types later in my attempts to reconstruct the evolution and phylogeny of the cell. I will draw attention now only to a few salient points. In the Protist cell the chromatin is not necessarily confined to the nucleus, but may occur also as extranuclear grains and fragments termed chromidia, scattered through the protoplasmic body; and the chromatin may be found only in the chromidial condition, a definite nucleus being temporarily or permanently absent. Further, when a true nucleus is present in the Protist body, it seldom contains a nucleolus of the same type as that seen in the nuclei of tissue-cells, that is to say, a mass of pure plastin, but in its place is found usually a conspicuous body which shows reactions agreeing more or less closely with those of chromatin and which consists of a plastin-basis more or less densely impregnated with chromatin. Such a body is termed a karyosome (or chromatin-nucleolus) to distinguish it from the true nucleoli (plastin-nucleoli) characteristic of tissue-cells. According as the plastin or the chromatin predominates in the composition of a karyosome, its reactions may resemble more nearly those of a true nucleolus in the one case, or those of chromatin in the other. The so-called karyosomatic type of nucleus is very common in the Protista, but by no means of invariable occurrence; in many cases the nucleus consists of a clump of small grains of chromatin, with no distinct karyosome, or with a karyosome which consists mainly of plastin. Thus two extreme types of nuclear structure can be distinguished and may be termed provisionally the karyosomatic type and the granular type, ignoring for the sake of convenience in nomenclature the types of structure transitional between the two; as, for example, types in which a distinct karyosome is seen together with more or fewer peripherally arranged grains of chromatin.

In either the karyosomatic or the granular type of Protist nucleus we may find great simplification of the complex type of nuclear structure seen in the tissue-cells of animals and plants. Thus in the first place a distinct nuclear membrane may be entirely absent and the chromatin-elements, whether occurring in the form of a compact karyosome or of a clump of grains, are lodged simply in a vacuole in the cytoplasm, that is to say in a cavity containing a watery fluid of nuclear sap in which the mass or masses of chromatin are suspended. It is a moot point, to which I shall return again, whether in nuclei of this simple type the linin-framework may sometimes be absent altogether, or whether it is invariably present in at least a rudimentary form, appearing as delicate threads (in optical section) extending from the chromatin-masses to the limiting wall of the nuclear vacuole, or between the grains of chromatin themselves. When such a framework can be detected, the nucleus acquires the appearance, in preserved preparations at least, of possessing a definite structure and is often termed a resting nucleus; many observations have shown, however, that the nucleus during life is undergoing continual internal movements and re-arrangements of its parts and is by no means at rest. The linin-framework cannot, therefore, be regarded in any way as a rigid skeleton, but must be interpreted as an alveolar framework similar to that of the general protoplasm and equally liable to movement, displacement, and change.

From this survey, necessarily most brief and superficial, of the manner in which the nuclei of Protists may vary from the type of nucleus described in the text-books, it is at once evident that the essential part of the nucleus is the chromatin, and that the other structural constituents of the nucleus, namely, membrane, framework, and plastin or nucleolar bodies, are to be regarded as accessory components built up round, or added to, the primary nuclear material, the chromatin. Even with regard to the nuclei of Metazoa it is maintained by Vejdovský that at each cell-generation the entire nucleus of the daughter-cell is produced from the chromosomes alone of the mother cell.[6] The simplest body which can be recognised as a nucleus, distinct from the chromidia scat-

[6] Walker, on the other hand, considers that 'it seems quite possible that the chromatin is merely a secretion of the linin.' (*Science Progress*, vol. vii. p. 641.) I doubt whether there are many cytologists who would admit this possibility, and I think that very few protistologists would assent to any such notion, since in the nuclei of Protista the linin-framework is in many cases very little in evidence, if present at all.

tered without order or arrangement throughout the protoplasmic body, is a mass of chromatin or a clump of chromatin-grains supported on a framework and lodged in a special vacuole in the cytoplasm. The complexity seen in the most perfect type of nucleus takes origin by progressive elaborations of, and additions to, a structure of this simple and primitive type.

This brings me to a point which I wish to emphasise most strongly, namely, that the conception of a true cell-nucleus is essentially a structural conception. A nucleus is not merely an aggregation of chromatin; it is not simply a central core of some chemical substance or material differing in nature from the remainder of the protoplasm. As Dobell has well expressed it, a pound of chromatin would not make a nucleus. The concepts 'nucleus' and 'chromatin' differ as do those of 'table' and 'wood.' Although chromatin is the one universal and necessary constituent entering into the composition of the cell-nucleus, a simple mass of chromatin is not a nucleus.[7] A true nucleus is a cell-organ, of greater or less structural complexity, which has been elaborated progressively in the course of the evolution of the cell; it is as much an organ of the cell as the brain is an organ of the human body. As a definite cell-organ, it performs in the life and economy of the cell definite functions, which it is the province of the cytologist to observe and to study, and if possible to elucidate and explain. As an organ of the cell, however, it has no homologue or analogue in the body of the multicellular animals or plants; there is no organ of the human body, taken as a whole, similar or comparable to the nucleus of the cell. Consequently, in studying the functions of the nucleus the human cytologist finds himself in the same difficult position that an intelligent living being lacking the sense of sight would be when trying to discover the function of visual organs in other organisms possessing that sense. There is no organ of known and understood functions with which the cytologist can compare the cell-nucleus directly.

The foregoing brief consideration of the nucleus leads me now to discuss in more detail the nature and properties of the essential nuclear substance, the so-called chromatin. To define, or characterise adequately, this substance is a difficult task. The name chromatin is derived from the fact that this substance has a peculiar affinity for certain dyes or stains, so that when a cell is treated with the appropriate colouring reagents—with so-called nuclear stains—the chromatin in the nucleus stands out sharply, by reason of being coloured in a different manner from the rest of the cell. In consequence, the statement is frequently made, in a loose manner and without reflection, that chromatin is recognised by its staining reactions, but in reality this is far from being true. When a preparation of an ordinary cell is made by the methods of technique commonly in use, the chromatin is recognised and identified by its position in a definite body with characteristic structure and relations to the cell as a whole, namely the nucleus, and this is equally true whether the chromatin has been stained or not. When the cell has been stained with one of the dyes ordinarily in use for colouring the chromatin, there are often seen in the cytoplasm grains that are coloured in exactly the same manner as the chromatin-grains lodged in the nucleus. Is an extranuclear grain which stains like chromatin to be identified, *ipso facto*, as chromatin? By no means; it may or it may not be chromatin. Simple inspection of a stained preparation is altogether inadequate to determine whether such a body is or is not chromatin. Any so-called chromatin-stain colours many bodies which may occur in a cell besides the chromatin, and it may be nceessary to try a great many

[7] Professor Armstrong writes: 'Every organism must possess some kind of nucleus, visible or invisible: some formative centre round which the various templates assemble that are active in directing the growth of the organism.' (*Science Progress*, vol. vii. p. 328.) I need hardly point out that a chemical nucleus of this kind is not in the least what the biologist or cytologist means by the term cell-nucleus. The one is a subjective postulate necessary for the comprehension of the activities of any speck of living matter or any portion, however minute, of a living organism; the other is a concrete structure, known to us by actual observation, and as much an integral part of the true cell, considered as a definite type of organism, as a backbone or its morphological equivalent is essential to the definition of a true vertebrate.

different stains before a combination is found which will differentiate a given cytoplasmic enclosure from a true chromatin-grain by its colour-reactions. The so-called volutin-grains, for example, which are found commonly in the cytoplasm of many Protists, are identified by the fact that they have a stronger affinity for 'chromatin-stains' than chromatin itself.

When, moreover, chromatin is compared with regard to its staining-reactions, both in different organisms, and in the same organism at different times, it is found to react very differently to one and the same stain. A striking example of this capriciousness is seen when a preserved film is made of the blood of some vertebrate which has nucleated blood-corpuscles, such as a bird or fish, and which contains also parasitic trypanosomes. It is easy to stain the nuclei of the blood-corpuscles with various stains, as for example carmine-stains such as picro-carmine or alum-carmine, which will not colour the nuclei of the trypanosomes in the slightest. Moreover, every cytologist knows that the 'chromaticity' of the chromatin varies enormously in different phases of the nuclear cycle of generation; it is often difficult to stain the chromatin in the 'resting' nucleus, but the first sign of impending nuclear division is a marked increase in the staining powers of the chromatin. There is no dye known which can be relied upon to stain chromatin always, or wherever it occurs. Methyl-green has been claimed to be the most reliable and certain of nuclear stains, but R. Hertwig, in his classical researches upon *Actinosphærium,* showed that it sometimes fails to stain chromatin. It is perfectly conceivable that there might be varieties of chromatin which could not be stained by any dye whatsoever.

I have felt bound to insist strongly upon the inadequacy of staining-methods for the detection and identification of chromatin, well known though these facts are to every cytologist, because here also I note a tendency amongst biological chemists to regard staining-properties as the sole criterion of chromatin. In reality such properties are of entirely secondary importance. To use the terminology of formal logic, staining-properties are an 'accident,' though it may be an 'inseparable accident,' of chromatin, not a 'difference' which can be used to frame a logical definition, *per genus et differentias,* of this substance. If chromatin were nothing more than 'stainable substance,' as Professor Armstrong terms it,[8] some of the most important results of cytological investigation would be deprived of all real significance and reduced to the merest futilities.

What then is the true criterion of the chromatin-substance of living organisms? From the chemical point of view the essential substance of the cell-nucleus would appear to be characterised by a complexity of molecular structure far exceeding that of any other proteins, as well as by certain definite peculiarities. Especially characteristic of chromatin is its richness in phosphorus-compounds, and it stands apart also from other cell-elements in its solvent reactions, for example, resistance to peptic digestion. E. B. Wilson, in his well-known treatise, has emphasised the 'cardinal fact . . . that there is a definite and constant contrast between nucleus and cytoplasm.' The outstanding feature of the nucleus is the constant presence in abundance of nuclein and nucleoproteins. Nuclein, which is probably identical with chromatin, is a complex albuminoid substance rich in phosphorus. It is the phosphorus-content of chromatin that is its most characteristic chemical peculiarity as contrasted with the cytoplasm. How far these features are common, however, to all samples of chromatin in all types of living organisms universally, cannot, I think, be stated definitely at present; at any rate, it is not feasible for a cytologist of these days to identify a granule in a living organism or cell as chromatin solely by its chemical reactions, although it is quite possible that at some future time purely chemical tests will be decisive upon this point—a consummation devoutly to be wished.

The only criterion of chromatin that is convincing to the present-day biologist is the test of its behaviour, that is to say, its relations to the life, activity, and development of the organism. I may best express my meaning by objective examples. If I make a preparation of *Arcella vulgaris* by suitable methods, I see the two conspicuous nuclei and also a ring of granules lying

[8] *Science Progress,* vol. vii. p. 327.

in the cytoplasm, stained in the same manner as the chromatin of the nuclei. Are these extranuclear granules to be regarded also as chromatin? Yes, most decidedly, because many laborious and detailed investigations have shown that from this ring of granules in *Arcella* nuclei can arise, usually termed 'secondary' nuclei for no other reason than that they arise *de novo* from the extranuclear chromatin and quite independently of the 'primary' nuclei. The secondary nuclei are, however, true nuclei in every respect, as shown by their structure, behaviour, and relations to the life-history of the organism; they may fuse as nuclei of gametes (pronuclei) in the sexual act and they become, with or without such fusion, the primary nuclei of future generations of *Arcella*; they then divide by karyokinesis when the organism reproduces itself in the ordinary way by fission, and are replaced in their turn by new secondary nuclei at certain crises in the life-history. In view of these facts it can be asserted without hesitation that the ring of staining granules in *Arcella* is composed of, or at least contains, true chromatin-grains, extranuclear chromatin for which R. Hertwig's term chromidia is now used universally. It is interesting to note that until the life-history of *Arcella* was studied in recent times the conspicuous ring of chromidia was generally overlooked and is not shown in some of the older pictures of the organism.

If, on the other hand, I make a preparation of some unidentified amœba occurring casually in pond-water or in an infusion, and find in its cytoplasm certain grains staining in same manner as the chromatin of the nucleus, it is quite impossible, without a knowledge of the life-history of the organism, to assert definitely that the grains in question are or are not true chromidia. They might equally well turn out to be volutin or any other substance that has an affinity for the particular chromatin-stains used in making the preparation.

The fact that at the present time the only decisive criterion of what is or is not chromatin is supplied only by its behaviour in the life-history and its relation to the organism, makes it much easier to identify the chromatin in some cases than in others. In those Protista or cells which contain, during the whole or a part of the life-history, one or more true nuclei, recognisable as such unmistakably by their structure and their characteristic relations to the reproductive and sexual phenomena of the organism, the chromatin can be identified with certainty. If chromidia occur in the cell-body in addition to true nuclei, or even if the nuclei are temporarily absent during certain crises of the life-history and the chromatin occurs then only in the form of chromidia, there is still no difficulty in identifying the scattered chromatin-grains by the fact that they contribute, soon or later, to the formation of nuclei.

On the other hand, in the simplest Protist organisms which do not contain definite, compact nuclei recognisable by their structure and behaviour, the identification of the chromatin may become correspondingly difficult. In the absence of definite chemical criteria the term chromatin acquires then a greater or less degree of vagueness and uncertainty of application, and it is not easy to avoid a tendency to a *petitio principii* in attempting to define or identify it. To a large extent we are thrown back upon the staining-reactions, which I have already shown to be very unreliable, backed up by analogies with those forms which possess definite nuclei. Since in the cells of all animals and plants, and in all Protista which possess a true nucleus, the chromatin is the one constituent which is invariably present, as I shall point out in more detail subsequently, there is at least a strong presumption, though not of course amounting to absolute proof, that it is present, or at least is represented by some similar and genetically homologous constituents, in the forms of simpler structure also. If then in Protista of primitive type we find certain grains which exhibit the characteristic staining-reactions of chromatin to be constantly present in the organism, grains which grow and divide as a preliminary to the organism multiplying by fission and which are partitioned amongst the daughter-organisms during the process of fission, so that each daughter-individual reproduces the structure of the parent-form from which it arose; then there is very strong *prima facie* evidence, to say the least, for regarding such grains as homologous with the chromatin-grains of ordinary cells.

Having now defined or explained, as well as I am able, the terms of which I am about to make use, I return to my main theme, the cell and its evolution. To summarise the points already discussed, a typical cell is a mass of protoplasm differentiated into two principal parts or regions, the cytoplasm and the nucleus, or, it may be, two or more nuclei. The cytoplasm may or may not contain chromatin-grains in addition to other enclosures, and may possess cell-organs of various kinds. The nucleus, highly variable in minute structure, possesses one invariable constituent, the chromatin-material in the form of grains and masses of various sizes.

The cell, therefore, in its complete and typical form, is an organism of very considerable complexity of structure and multiplicity of parts. The truth of this proposition is sufficiently obvious even from simple inspection of the structural details revealed by the microscope in cells in the so-called 'resting condition,' but still more so from a study of their activities and functions. The vital processes exhibited by the cell indicate a complexity of organisation and a minuteness in the details of its mechanism which transcend our comprehension and baffle the human imagination, to the same extent as do the immensities of the stellar universe. If such language seems hyperbolic, it is but necessary to reflect on some of the established discoveries of cytology, such as the extraordinary degree of complication attained in the process of division of the nucleus by karyokinesis, or the bewildering series of events that take place in the nuclei of germ-cells in the processes of maturation and fertilisation. Such examples of cell-activity give us, as it were, a glimpse into the workshop of life and teach us that the subtlety and intricacy of the cell-microcosm can scarcely be exaggerated.

On the assumption that an organism so complex and potent was not created suddenly, perfect and complete as it stands, but arose, like all other organisms, by progressive evolution and elaboration of some simpler form and type of structure, it is legitimate to inquire which of the various parts of the cell are the older and more primitive and which are more recent acquisitions in the course of evolution. But it must be clearly pointed out, to start with, that the problem posed in such an inquiry is perfectly distinct from, and independent of, another point which has often been discussed at length, namely, the question whether any parts of the cell, and if so which parts, are to be regarded as 'living' or 'active' in distinction to other parts which are to be regarded as 'not-living' or 'passive.' This discussion, in my opinion, is a perfectly futile one, of which I intend to steer clear.

We may agree that in any given cell or living organism, simple or complex in structure, all the parts are equally 'living' and equally indispensable for the maintenance of life, or at least for the continuance of the vital functions in the normal, specific manner, without losing the right to inquire which of those parts are the phylogenetically older. A simple analogy will serve to point my meaning. A man could not continue to live for long if deprived either of his brain, his digestive tract, his lungs, his heart, or his kidneys, and each of these organs is both 'living' in itself and at the same time an integral part of the entire organisation of the human body; yet no one would think of forbidding comparative anatomists to discuss, from the data at their command, which of these organs appeared earlier, and which later, in the evolution of the phylum Vertebrata. Moreover, speculative though such discussions must necessarily be, there is no one possessing even a first-year student's knowledge of the facts who would controvert the statement that the digestive tract of man is phylogenetically older than the lung. Speculative conclusions are not always those that carry the least conviction.

The evolution of the cell may be discussed as a morphological problem of the same order as that of the phylogeny of any other class or phylum of living beings, and by the same methods of inquiry. In the first place there is the comparative method, whereby different types of cell-structure can be compared with one another and with organisms in which the cell-structure is imperfectly developed, in order to determine what parts are invariable and essential and what are sporadic in occurrence and of secondary importance, and if possible to arrange the various structural types in one or more evolutionary series. Secondly there is the developmental or ontogenetic method, the study of the mode and sequence of the formation of the parts of the cell as they come into

existence during the life-history of the organism. Both these methods, which are founded mainly on observation, require to be checked and controlled by the experimental methods of investigating both the functions and behaviour of the organism and of its parts.

So long as cytologists limit their studies to the cells building up the tissues of the higher animals and plants, the comparative method has a correspondingly limited scope, and that of the ontogenetic method is even more restricted. Both methods receive at once, however, an enormously extended range when the Protista are taken into consideration. Then, moreover, we see the dawning possibility of another method of investigation, that, namely, of the chemical evolution of the organisms. Already some of the simpler Protista, the Bacteria, are characterised and classified largely by their chemical activities; but in more complex organisms, in those which have attained complete cell-structure, such as Protozoa, the data of chemistry do not as yet supply the evolutionist with a helpful method of investigation.

The problem of cell-evolution may be attacked by the help of the methods outlined in the foregoing remarks, beginning with the consideration of the primary structural differentiation of the typical cell, the distinction of nucleus, or rather chromatin, and cytoplasm. Since all cells known to us exhibit this differentiation, we have three possibilities as regards the manner in which it has come about, which may be summarised briefly as follows: either the cytoplasmic and chromatinic constituents of the cell have arisen as differentiations of some primitive substance, which was neither the one nor the other; or one of these two substances is a derivative of the other, in the course of evolution, either cytoplasm of chromatin, or chromatin of cytoplasm.

The idea of a primitive, undifferentiated protoplasmic substance was first put forward by Haeckel, who employed for it the term 'plasson' invented by Van Beneden[9] to denote 'la substance constitutive du corps des Monères et des cytodes . . . le substance formative par excellence.' The simplest elementary organisms were not cells, but cytodes, 'living independent beings which consist entirely of a particle of plasson; their quite homogeneous or uniform body consists of an albuminous substance which is not yet differentiated into karyoplasm and cytoplasm, but possesses the properties of both combined.'[10] It is emphasised[11] that a sharp distinction must be drawn between protoplasm and plasson, the latter being a homogeneous albuminous formative substance ('Bildungsstoff') corresponding to the 'Urschleim' of the older Nature-philosophy.

Haeckel, as was usual with him, did not content himself with putting forward his ideas as abstract speculations, but sought to provide them with a concrete and objective foundation by professing to have discovered, and describing in detail, living and existing organisms which were stated to remain permanently in the condition of cytodes. In consequence, a purely speculative notion was permitted to masquerade for many years under the false appearance of an objective phenomenon of Nature, until the error was discovered gradually and the phantom banished from the accepted and established data of biology. Organisms supposed to be of the nature of cytodes constituted Haeckel's systematic division Monera, of which there were supposed to be two subdivisions, the Phytomonera and the Zoomonera. The Phytomonera were stated to have the plasson coloured green and to live in a plant-like manner; the Zoomonera were colourless amœboid masses of plasson which nourished themselves in the animal manner. The Bacteria were also included by Haeckel in his Monera, apparently, or at all events ranked as cytodes.[12] Most importance, however, was attributed by Haeckel to the large amœboid forms of Monera, described as without nuclei or contractile vacuoles, but as representing simply structureless contractile masses of albumin ('Eiweiss'), perfectly homogeneous;[13] examples of these were announced to exist under the names 'Protamœba' and 'Protogenes,' denoting forms of life which Haeckel claimed

[9] *Bull. de l'Acad. Roy. de Belgique*, second series, vol. xxxi. (1871), p. 346.
[10] *Anthropogenie*, sixth edition, Leipzig, 1910, p. 119.
[11] *Ibid.* p. 532.
[12] *Ibid.* p. 119.
[13] See his *Prinzipien der generellen Morphologie*, Berlin, 1906, p. 61.

to have discovered, but which have never been found again by any other naturalist. These organisms, as described by Haeckel, were by no means such as the modern microscopist would call minute; on the contrary, they were relatively large, and some of the forms added to the Monera by Haeckel's contemporaries might even be termed gigantic, as, for example, the supposed organism *Bathybius*, discovered in the bottles of the *Challenger* Expedition, which was believed to cover large areas of the floor of the ocean with a layer of primordial protoplasm, but which proved finally to be a precipitation by alcohol of the gypsum in sea-water.

The theory of plasson and of the cytodes of Haeckel may be considered first from the purely speculative standpoint of the origin of the living substance, a problem with which I wish to become entangled here as little as possible, since it is my object to confine myself so far as possible to deductions and conclusions that may be drawn from known facts and concrete data of observation and experiment. If, however, we postulate a chemical evolution of protoplasm, and believe that every degree of complexity exists, or at least has existed, between the simplest inorganic compounds and the immensely complicated protein-molecules of which the living substance is composed, then no doubt chemical compounds may have existed which in some sense were intermediate in their properties between the two constituents, cytoplasm and chromatin, found in all known samples of the living substance of organisms. In this sense and on such a hypothesis, a substance of the nature of plasson may perhaps be recognised or postulated at some future time by the biochemist, but this is a subject which I am quite incompetent to discuss. To the modern biologist, who can deal only with living things as he knows them, Haeckel's plasson must rank as a pure figment of the imagination, altogether outside the range of practical and objective biology at the present time. All visible living things known and studied up to the present consist of protoplasm, that is to say, of an extremely heterogeneous substance of complex structure, and no living organism has been discovered as yet which consists of homogeneous structureless albuminous substance. Van Beneden, who is responsible for the word plasson, though not for the cytode-theory, was under the impression that he had observed a non-nucleated homogeneous cytode-stage in the development of the gregarine of the lobster, *Gregarina* (*Porospora*) *gigantea*. Without entering into a detailed criticism of Van Beneden's observations upon this form, it is sufficient to state that the development of gregarines is now well known in all its details, and that in all phases of their life-cycle these organisms show the complete cell-structure, and are composed of nucleus and cytoplasm. Moreover, all those organisms referred by Haeckel to the group Monera which have been recognised and examined by later investigators have been found to consist of ordinary cytoplasm containing nuclei or nuclear substance (chromatin). In the present state of biological knowledge therefore, the Monera as defined by Haeckel must be rejected and struck out of the systematic roll as a non-existent and fictitious class of organisms.

Since no concrete foundation can be found for the view that cytoplasm and chromatin have a common origin in the evolution of living things, we are brought back to the view that one of them must have preceded the other in phylogeny. The theories of evolution put forward by Haeckel and his contemporaries, if we abolish from them the notion of plasson and substitute for it that of ordinary protoplasm, would seem to favour rather the view that the earliest forms of life were composed of a substance of the nature rather of cytoplasm, and that the nuclear substance or chromatin appeared later in evolution as a product or derivative of the cytoplasm. I have myself advocated a view diametrically opposite to this, and have urged that the chromatin-substance is to be regarded as the primitive constituent of the earliest forms of living organisms, the cytoplasmic substance being a later structural complication. On this theory the earliest form of living organism was something very minute, probably such as would be termed at the present day 'ultra-microscopic.' After I had urged this view in the discussion on the origin of life at the Dundee Meeting of the British Association in 1912 a poem appeared in *Punch*,[14] dividing biologists into 'cytoplasmists' and 'chromatinists.' I

[14] Vol. cxliii. p. 245.

must confess myself still a whole-hearted chromatinist. But before I consider this point I may refer briefly to some other speculations that have been put forward with regard to the nature of the earliest form of life. It is manifestly quite impossible that I should undertake here to review exhaustively all the theories and speculations with regard to the origin of life and the first stages in its evolution that have been put forward at different times. I propose to limit myself to the criticism of certain theories of modern times which, recognising the fundamental antithesis between chromatin and cytoplasm, regard these two cell-constituents as representing types of organisms primitively distinct, and suggest the hypothesis that true cells have arisen in the beginning as a process of symbiosis between them. Boveri, whose merits as a cytologist need no proclamation by me, was the first I believe to put forward such a notion; he enunciated the view that the chromosomes were primitively independent elementary organisms which live symbiotically with protoplasm, and that the organism known as the cell arose from a symbiosis between two kinds of simple organisms, 'Monera.'[15]

A similar idea lies at the base of the remarkable and ingenious speculations of Mereschkowsky,[16] who assumes a double origin for living beings from two sorts of protoplasm, supposed not only to differ fundamentally in kind but also to have had origins historically distinct. The first type of protoplasm he terms mycoplasm,[17] which is supposed to have come into existence during what he calls the third epoch[18] of the earth's history, at a time when the crust of the earth had cooled sufficiently for water to be condensed upon it, but when the temperature of the water was near boiling-point; consequently the waters of the globe were free from oxygen, while saturated with all kinds of mineral substances. The second type of protoplasm was amœboplasm, the first origin of which is believed to have taken place during a fourth terrestrial epoch when the waters covering the globe were cooled down below 50° C., and contained dissolved oxygen but fewer mineral substances. Corresponding with the differences of the epoch and the conditions under which they arose, Mereschkowsky's two types of protoplasm are distinguished by sharp differences in their nature and constitution.

Mycoplasm, of which typical examples are seen in bacteria, in the chromatin-grains of the nucleus and the chromatophores of plant-cells, is distinguished from amœboplasm, which is simply cytoplasm, by the following points. (1) Mycoplasm can live without oxygen, and did so in the beginning at its first appearance when the temperature of the hydrosphere was too high for it to have contained dissolved oxygen; only at a later period, when the temperature became low enough for the water to contain oxygen in solution, did some of the forms begin to adapt themselves to these conditions, and became secondarily facultative or obligate aerobes. Amœboplasm, on the other hand, cannot exist without a supply of oxygen. (2) Mycoplasm can support temperatures of 90° C. or even higher; amœboplasm cannot support a temperature higher than 45° C. or 50° C. (3) Mycoplasm is capable of building up albumins and complex organic substances from inorganic materials; amœboplasm is incapable of doing so, but requires organic food. (4) Mycoplasm has restricted powers of locomotion and is incapable of amœboid movement, or of forming the contractile vacuoles seen commonly in amœboplasm. (5) Mycoplasm, in contrast to amœboplasm, is rich in phosphorus and nuclein. (6) Mycoplasm is extraordinarily resistant to poisons and utilises as food many substances that are extremely deadly to amœboplasm, such as prussic acid, strychnine, and morphia.

[15] *Fide* Vejdovský, *l.c.* I have not had access to the work of Boveri, in which he is stated to have put forward these ideas.

[16] Mereschkowsky, C., 'Theorie der zwei Plasmaarten als Grundlage der Symbiogenesis, einer neuen Lehre von der Entstehung der Organismen. *Biol. Centralblatt*, xxx. 1910, pp. 278-303, 321-347, 353-367.

[17] The term mycoplasm used by Mereschkowsky must not be confounded with the similar word used by Eriksson and other botanists in reference to the manner in which Rust-Fungi permeate their hosts.

[18] In the first epoch the earth was an incandescent mass of vapour; in the second it had a firm crust, but the temperature was far too high to permit of the condensation of water-vapour upon its surface.

(7) Amongst minor differences, mycoplasm is characterised by the presence of iron in the combined state and possesses a far more complicated structure than amœboplasm, a peculiarity which enables mycoplasmic cell-elements (chromosomes) to function as the bearers of hereditary qualities.

The course of the evolution of living beings, according to Mereschkowsky, was as follows. The earliest forms of life were 'Biococci,' minute ultra-microscopic particles of mycoplasm, without organisation, capable of existing at temperatures near boiling-point and in the absence of oxygen, possessing the power of building up proteins and carbohydrates from inorganic materials, and very resistant to strong mineral salts and acids and to various poisons. From the Biococci arose in the first place the Bacteria, which for a time were the only living inhabitants of the earth. Later, when the temperature of the terrestrial waters had been lowered below 50° C., and contained abundant organic food in the shape of Bacteria, amœboplasm made its appearance in small masses as non-nucleated Monera which crept in an amœboid manner on the floor of the ocean and devoured Bacteria.

The next step in evolution is supposed to have been that, in some cases, micrococci ingested by the Monera resisted digestion by them and were enabled to maintain a symbiotic existence in the amœboplasm. At first the symbiotic micrococci were scattered in the Moneran body, but later they became concentrated at one spot, surrounded by a membrane, and gave rise to the cell-nucleus. In this way, by a 'symbiogenesis' or process of symbiosis between two distinct types of organisms, Mereschkowsky believes the nucleated cell to have arisen, an immense step forward in evolution, since the locomotor powers of the simple and delicate Monera were now supplemented by the great capability possessed by the Bacteria of producing ferments of the most varied kinds.

Meanwhile it is supposed that the free Bacteria continued their natural evolution and gave rise to the Cyanophyceæ, and to the whole group of Fungi. The plant-cell came into existence by a further process of symbiogenesis, in that some of the Cyanophyceæ, red, brown, or green in colour, became symbiotic in nucleated cells, for the most part flagellates, in which they established themselves as the chromatophores or chlorophyll-corpuscles. In this way Mereschkowsky believes the vegetable cell to have come into existence, and the evolution of the Vegetable Kingdom to have been started, as a double process of symbiosis. Those amœboid or flagellated organisms, on the other hand, which formed no symbiosis with Cyanophyceæ, continued to live as animals and started the evolution of the Animal Kingdom.

As a logical deduction from this theory of the evolution of living beings, Mereschkowsky classifies organisms generally into three groups or Kingdoms: first the Mycoidea, comprising Bacteria, Cyanophyceæ, and Fungi, and in which no symbiosis has taken place; secondly, the Animal Kingdom, in which true cells have arisen by a simple symbiosis of mycoplasm (chromatin) and amœboplasm (cytoplasm); thirdly, the Vegetable Kingdom, in which true cells have entered upon an additional symbiosis with Cyanophyceæ, chromatophores or chlorophyll-corpuscles.

Interesting and suggestive as are the speculations of Mereschkowsky, they are nevertheless open to criticism from many points of view. I will not enter here into criticisms which I regard as beyond my competence. It is for botanists to pronounce upon the notion that Bacteria, Cyanophyceæ, and Fungi can be classified together as a group distinct from all other living beings; to decide whether the protoplasm of the Cyanophyceæ and Fungi can be regarded as consisting of mycoplasm alone, and not of a combination of nuclei and cytoplasm, such as is found in true cells and represents, according to Mereschkowsky, a symbiosis of mycoplasm and amœboplasm. I think I am right in saying that botanists are agreed in regarding Fungi as derived from green algæ, and as possessing nuclei similar to those of the higher plants. As a zoologist the point that strikes me most is the absence of any evidence that true Monera, organisms consisting of cytoplasm alone, exist or could ever have existed. Mereschkowsky supposes that when the Monera came into being they maintained their existence by feeding upon Bacteria. In order to digest Bacteria, however, the Monera must have been capable of producing ferments, and therefore did not acquire this power only as the result of symbiosis with Bacteria, unless it be assumed

that the symbiosis came about at the instant that amœboplasm came into existence. There is, however, no evidence that cytoplasm by itself can generate ferments. All physiological experiments upon the digestion of Protozoa indicates that the cytoplasmic body, deprived of the nucleus, cannot initiate the digestive process. Consequently the existence of purely cytoplasmic organisms would seem to be an impossibility.

For my part, I am unable to accept any theory of the evolution of the earliest forms of living beings which assumes the existence of forms of life composed entirely of cytoplasm without chromatin. All the results of modern investigations into the structure, physiology, and behaviour of cells on the one hand, and of the various types of organisms grouped under the Protista, on the other hand—the combined results, that is to say, of cytology and protistology—appear to me to indicate that the chromatin-elements represent the primary and original living units or individuals, and that the cytoplasm represents a secondary product. I will summarise briefly the grounds that have led me to this conviction, and will attempt to justify the faith that I hold; but first I wish to discuss briefly certain preliminary considerations which seem to me of great importance in this connection.

It is common amongst biologists to speak of 'living substance,' this phrase being preceded by either the definite or the indefinite article—by either 'the' or 'a.' If we pause to consider the meaning of the phrase, it is to be presumed that those who make use of it employ the term 'substance' in the usual sense to denote a form of matter to which some specific chemical significance can be attached, which could conceivably be defined more or less strictly by a chemist, perhaps even reduced to a chemical formula of some type. But the addition of the adjective 'living' negatives any such interpretation of the term 'substance,' since it is the fundamental and essential property of any living being that the material of which it is composed is in a state of continual molecular change and that its component substance or substances are inconstant in molecular constitution from moment to moment. When the body of a living organism has passed into a state of fixity of substance, it has ceased, temporarily or permanently, to behave as a living body; its fires are banked or extinguished. The phrase 'living substance' savours, therefore, of a *contradictio in adjecto*; if it is 'living' it cannot be a 'substance,' and if it is a 'substance' it cannot be 'living.'

As a matter of fact, the biologist, when dealing with purely biological problems, knows nothing of a living substance or substances; he is confronted solely by living individuals, which constitute his primary conceptions, and the terms 'life' and 'living substance' are pure abstractions. Every living being presents itself to us as a sharply-limited individual, distinct from other individuals and constituting what may be termed briefly a microcosmic unit, inasmuch as it is a unity which is far from being uniform in substance or homogeneous in composition, but which, on the contrary, is characterised by being made up of an almost infinite multiplicity of heterogeneous and mutually interacting parts. We recognise further that these living individuals possess invariably specific characteristics; two given living individuals may be so much alike that we regard them as of the same kind or 'species,' or they may differ so sharply that we are forced to distinguish between them specifically. Living beings are as much characterised by this peculiarity of specific individuality as by any other property or faculty which can be stated to be an attribute of life in general, and this is true equally of the simplest or the most complex organisms; at least we know of no form of life, however simple or minute, in which the combined features of individuality and specificity are not exhibited to the fullest extent. A living organism may be so minute as to elude direct detection entirely by our senses, even when aided by all the resources furnished by modern science; such an organism will, nevertheless, exhibit specific properties or activities of an unmistakable kind, betraying its presence thereby with the utmost certainty. The organisms causing certain diseases, for example, are ultra-microscopic, that is to say, they have not been made visible as yet, and an exact description or definition cannot be given of them at the present time; yet how strongly marked and easily distinguishable are the specific effects produced by the organisms causing respectively measles and small-pox, for instance, each, moreover, remaining strictly true and constant to its

specific type of activity; the organism, whatever its nature may be, which causes measles cannot give rise to small-pox, nor *vice versa*, but each breeds as true to type as do lions and leopards.

The essential and distinctive characteristic of a living body of any kind whatsoever is that it exhibits while it lives permanence and continuity of individuality or personality, as manifested in specific behaviour, combined with incessant change and lability of substance; and further, that in reproducing its kind, it transmits its specific characteristics, with, however, that tendency to variability which permits of progressive adaptation and gradual evolutionary change. It is the distinctively vital property of specific individuality combined with 'stuff-change' (if I may be allowed to paraphrase a Teutonic idiom) which marks the dividing line between Biochemistry and Biology. The former science deals with substances which can be separated from living bodies, and for the chemist specific properties are associated with fixity of substance; but the material with which the biologist is occupied consists of innumerable living unit-individuals exhibiting specific characteristics without fixity of substance. There is no reason to suppose that the properties of a given chemical substance vary in the slightest degree in space or time; but variability and adaptability are characteristic features of all living beings. The biochemist renders inestimable services in elucidating the physico-chemical mechanisms of living organisms; but the problem of individuality and specific behaviour, as manifested by living things, is beyond the scope of his science, at least at present. Such problems are essentially of distinctively vital nature and their treatment cannot be brought satisfactorily into relation at the present time with the physico-chemical interactions of the substances composing the living body. It may be that this is but a temporary limitation of human knowledge prevailing in a certain historical epoch, and that in the future the chemist will be able to correlate the individuality of living beings with their chemico-physical properties, and so explain to us how living beings first came into existence; how, that is to say, a combination of chemical substances, each owing its characteristic properties to a definite molecular composition, can produce a living individual in which specific peculiarities are associated with matter in a state of flux. But it is altogether outside the scope and aim of this address to discuss whether the boundary between biochemistry and biology can be bridged over, and if so, in what way. I merely wish to emphasise strongly that if a biologist wishes to deal with a purely biological problem, such as evolution or heredity, for example, in a concrete and objective manner, he must do so in terms of living specific individual units. It is for that reason that I shall speak, not of the chromatin-substance but of chromatinic elements, particles or units, and I hope that I shall make clear the importance of this distinction.

To return now to our chromatin; I regard the chromatinic elements as being those constituents which are of primary importance in the life and evolution of living organisms mainly for the following reasons: the experimental evidence of the preponderating physiological *rôle* played by the nucleus in the life of the cell; the extraordinary individualisation of the chromatin-particles seen universally in living organisms, and manifested to a degree which raises the chromatinic units to the rank of living individuals exhibiting specific behaviour, rather than that of mere substances responsible for certain chemico-physical reactions in the life of the organism; and last, but by no means least, the permanence and, if I may use the term, the immortality of the chromatinic particles in the life-cycle of organisms generally. I will now deal with these points in order; my arguments relate, in the first instance, to those organisms in which the presence of true cell-nuclei renders the identification of the chromatin-elements certain, as pointed out above, but if the arguments are valid in such cases they are almost certainly applicable also to those simpler types of organisms in which the identification of chromatin rests on a less secure foundation.

The results obtained by physiological experiments with regard to the functions of the nuclear and cytoplasmic constituents of the cell are now well known and are cited in all the text-books. It is not necessary, therefore, that I should discuss them in detail I content myself with quoting a competent and impartial summary of the results obtained:

'A fragment of a cell deprived of its nucleus may live for a considerable time and manifest the power of co-ordinated movements without perceptible impairment. Such a mass of protoplasm is, however, devoid of the powers of assimilation, growth, and repair, and sooner or later dies. In other words, those functions that involve destructive metabolism may continue for a time in the absence of the nucleus; those that involve constructive metabolism cease with its removal. There is, therefore, strong reason to believe that the nucleus plays an essential part in the constructive metabolism of the cell, and through this is especially concerned with the formative processes involved in growth and development. For these and many other reasons . . . the nucleus is generally regarded as a controlling centre of cell-activity, and hence a primary factor in growth, development, and the transmission of specific qualities from cell to cell, and so from one generation to another.' [19]

I may add here that the results of the study of life-cycles of Protozoa are entirely in harmony with this conception of the relative importance of nuclear —that is chromatinic—and cytoplasmic cell-constituents, since it is not infrequent that in certain phases of the life-cycle, especially in the microgamete-stages, the cytoplasm is reduced, apparently, to the vanishing point, and the body consists solely of chromatin, so far as can be made out. In not one single instance, however, has it been found as yet that any normal stage in the developmental cycle of organisms consists solely of cytoplasm without any particles of chromatin.

While on the subject of physiological experiment, there is one point to which I may refer. Experiments so far have been carried on with Protozoa possessing definite nuclei. It is very desirable that similar experiments should be conducted with forms possessing chromidia in addition to nuclei, in order to test the physiological capabilities of chromatin-particles not concentrated or organised. *Arcella* would appear to be a very suitable form for such investigations. This is a point to which my attention was drawn by my late friend Mr. C. H. Martin, who has lost his life in his country's service.

I have mentioned already in my introductory remarks that the only reliable test of chromatin is its behaviour, and the whole of modern cytological investigation bears witness to the fact that the chromatinic particles exhibit the characteristic property of living things generally, namely, individualisation combined with specific behaviour. In every cell-generation in the bodies of ordinary animals and plants the chromatin-elements make their appearance in the form of a group of chromosomes, not only constant in number for each species, but often exhibiting such definite characteristics of size and form, that particular, individual chromosomes can be recognised and identified in each group throughout the whole life-cycle. Each chromosome is to be regarded as an aggregate composed of a series of minute chromatinic granules or chromioles, a point which I shall discuss further presently. Most striking examples of the individualisation of chromosomes have been made known recently by Dobell and Jameson [20] in Protozoa. Thus in the Coccidian genus *Aggregata* six chromosomes appear at every cell-generation, each differing constantly in length if in the extended form, or in bulk if in the contracted form, so that each of the six chromosomes can be recognised and denoted by one of the letters *a* to *f* at each appearance, *a* being the longest and *f* the shortest.

Even more remarkable than the relation of the chromosome to cell-reproduction is their behaviour in relation to sexual phenomena. In the life-cycles of Metazoa the sexual act consists of the fusion of male and female pronuclei, each containing a definite and specific number of chromosomes, the same number usually, though not always, in each pronucleus. It has been established in many cases, and it is perhaps universally true, that in the act of fertilisation the male and female chromosomes remain perfectly distinct and separate in the synkaryon or nucleus formed by the union of the two pronuclei, and, moreover, that they continue to maintain and to propagate their distinct individuality in every subsequent cell-generation of the multicellular organism produced as a result of the sexual act. In this way, every cell of the body contains in its nucleus distinct chromatinic elements which are derived from

[19] E. B. Wilson, *The Cell*, second edition, 1911, pp. 30 and 31.
[20] *Proc. Roy. Soc.* (B), vol. 89. (*In the Press.*)

both male and female parents and which maintain unimpaired their distinct and specific individuality through the entire life-cycle. This distinctness is apparent at least in the germ-cell-cycle of the organism, but may be obscured by secondary changes in the nuclei of the specialised tissue-cells.

Only in the very last stage of the life-cycle do the group of male and female chromosomes modify their behaviour in a most striking manner. In the final generation of oogonia or spermatogonia, from which arise the oocytes and spermatocytes which in their turn produce the gamete-cells, it is observed that the male and female chromosomes make a last appearance in their full number, and then fuse in pairs, so as to reduce the number of chromosomes to half that previously present.

In *Aggregata* also Dobell and Jameson have shown that the union of the pronuclei in fertilisation brings together two sets each of six chromosomes, and that these then fuse with one another in pairs according to type, that is to say *a* with *a*, *b* with *b*, *c* with *c*, and so on. Analogous phenomena have been demonstrated also in the gregarine *Diplospora*. We have here a difference in detail, as compared with the Metazoa, in that the fusion takes place at the fertilisation and not as the first step in the maturation of the germ-cells; but in both cases alike the fusion of chromatin-elements individually distinct and exhibiting specific characteristics is to be regarded as the final consummation of the sexual act, though long deferred in the Metazoan life-cycle.

As Vejdovský has pointed out, there can be no more striking evidence of the specific individuality of the chromosomes than their fusion or copulation in relation to the sexual act. Is there any other constant element or constituent of living organisms exhibiting to anything like the same degree the essentially vital characteristics of individuality manifested in specific behaviour? If there is, it remains to be discovered.

I come now to the question of the permanence and immortality, in the biological sense of the word, of the chromatinic particles, which may be summarily stated as follows: the chromatinic particles are the only constituents of the cell which maintain persistently and uninterruptedly their existence throughout the whole life-cycle of living organisms universally.

I hope I shall not be misunderstood when I enunciate this apparently sweeping and breathless generalisation. I am perfectly aware that in the life-cycle of any given species of organism there may be many cell-constituents besides the chromatin-particles that are propagated continuously through the whole life-cycle; but cell-elements which appear as constant parts of the organisation of the cell throughout the life-cycle in one type of organism may be wanting altogether in other types. With the exception of the chromatin-particles there is no cell-constituent that can be claimed to persist throughout the life-cycles of organisms universally. To take some concrete examples; the cytoplasmic grains known as mitochondria or chrondriosomes have been asserted to be persistent elements throughout the germ-cycle of Metazoa, and the function of being the bearers of hereditary tendencies has been ascribed to them. But Vejdovský[21] flatly denies the alleged continuity in cases investigated by him, and though chrondriosomes have been described in some Protozoa, there is no evidence whatever that they are of universal occurrence in Protista. Centrosomes, intranuclear or extranuclear, have been stated to be constant cell-components in some organisms; whether that is true or not it seems quite certain that in many organisms the cells are entirely without centrosomic bodies of any kind, as for example in the whole group of Phanerogams. So it is with any other cell-constituent that can be named. It may be that this is only the result of our incomplete knowledge at the present time. I am prepared, however, to challenge anyone to name or to discover any cell-constituent, other than the chromatinic particles, which are present throughout the life-cycle, not merely of some particular organism, but of organisms universally.

In this feature of continuity the chromatin-constituents of the cell present a remarkable analogy with the germ-plasm of Metazoa. Just as the germ-cells of Metazoa go on in an uninterrupted, potentially everlasting series of cell-generations, throwing off, as it were, at each sexual crisis a soma which is doomed to but a limited lease of life, during which it furnishes a nutritive

[21] *L.c.* pp. 77-89.

environment for further generations of germ-cells; so in the series of cell-generations themselves, whether in the germ-cell-cycles of Metazoa or in the life-cycles of Protista the chromatin-particles maintain an uninterrupted propagative series within a cell-body of which the various parts have a limited duration of existence, making their appearance, flourishing for a time, and disappearing again. This analogy between the chromatin of cells and the germ-plasm of multicellular organisms becomes still more marked when we find that in many Protozoa the chromatin may undergo a specialisation into generative and trophic chromatin, the former destined to persist from one life-cycle to another, the latter destined to control cell-activities merely during one cycle, without persisting into the next. The differentiation of generative and trophic chromatin is now well known to occur in many Protozoa, and in its most extreme form, as seen in the Infusoria, it is expressed in occurrence of two distinct nuclei in the cell-body.

To recapitulate my argument in the briefest form; the chromatinic constituents of the cell contrast with all the other constituents in at least three points: physiological predominance, especially in constructive metabolism; specific individualisation; and permanence in the sense of potential biological immortality. Any of these three points, taken by itself, is sufficient to confer a peculiar distinction, to say the least, on the chromatin-bodies; but taken in combination they appear to me to furnish overwhelming evidence for regarding the chromatin-elements as the primary and essential constituents of living organisms, and as representing that part of a living body of any kind which can be followed by the imagination, in the reverse direction of the propagative series, back to the very starting-point of the evolution of living beings.

In the attempt to form an idea as to what the earliest type of living being was like, in the first place, and as to how the earliest steps in its evolution and differentiation came about, in the second place, we have to exercise the constructive faculty of the imagination guided by such few data as we possess. It is not to be expected, therefore, that agreement can be hoped for in such speculations; it would indeed be very undesirable, in the interests of science, that there should be no conflict of opinion in theories which, by their very nature, are beyond any possibility of direct verification at the present time The views put forward by any man do but represent the visions conjured up by his imagination, based upon the slender foundation of his personal knowledge, more or less limited, or intuition, more or less fallacious, of an infinite world of natural phenomena. Consequently such views may be expected to diverge as widely as do temperaments. If, therefore, I venture upon such speculations, I do so with a sense of personal responsibility and as one wishing to stimulate discussion rather than to lay down dogma.

To me, therefore, the train of argument that I have set forth with regard to the nature of the chromatinic constituents of living organisms appear to lead to the conclusion that the earliest living beings were minute, possibly ultra-microscopic particles which were of the nature of chromatin. How far the application of the term chromatin to the hypothetical primordial form of life is justified from the point of view of substance, that is to say in a biochemical sense, must be left uncertain. In using the term chromatin I must be understood to do so in a strictly biological sense, meaning thereby that these earliest living things were biological units or individuals which were the ancestors, in a continuous propagative series, of the chromatinic grains and particles known to us at the present day as universally-occurring constituents of living organisms. Such a conception postulates no fixity of chemical nature; on the contrary, it implies that as substance the primitive chromatin was highly inconstant, infinitely variable, and capable of specific differentiation in many divergent directions.

For these hypothetical primitive organisms we may use Mereschkowsky's term biococci. They must have been free-living organisms capable of building up their living bodies by synthesis of simple chemical compounds. We have as yet no evidence of the existence of biococci at the present time as free-living organisms; the nearest approach to any such type of living being seems to be furnished by the organisms known collectively as Chlamydozoa, which up to the present have been found to occur only as pathogenic parasites. In view,

however, of the minuteness and invisibility of these organisms, it is clear that they could attract attention only by the effects they produce in their environment. Consequently the human mind is most likely to become aware in the first instance of those forms which are the cause of disturbance in the human body. If free-living forms of biococci exist, as is very possible and even probable, it is evident that very delicate and accurate methods of investigation would be required to detect their presence.

I am well aware that the nature and even the existence of the so-called Chlamydozoa is uncertain at the present time, and I desire to exercise great caution in basing any arguments upon them. In the descriptions given of them, however, there are some points which, if correctly stated, seem to me of great importance. They are alleged to appear as minute dots, on the borderline of microscopic visibility or beyond it; they are capable of growth, so that a given species may be larger or smaller at different times; their bodies stain with the ordinary chromatin-stains; and they are stated to reproduce themselves by a process of binary fission in which the body becomes dumbbell-shaped, appearing as two dots connected by a slender thread, which is drawn out until it snaps across and then the broken halves of the thread are retracted into the daughter-bodies. This mode of division, strongly reminiscent of that seen in centrioles, appears to me to permit of certain important conclusions with regard to the nature of these bodies; namely, that the minute dot of substance has no firm limiting membrane on the surface and that it is of a viscid or semi-fluid consistence.

If it be permissible to draw conclusions with regard to the nature of the hypothetical biococci from the somewhat dubious, but concrete data furnished by the Chlamydozoa, the following tentative statements may be postulated concerning them. They were (or are) minute organisms, each a speck or globule of a substance similar in its reactions to chromatin. Their substance could be described as homogeneous with greater approach to accuracy than in the case of any other living organism, but it is clear that no living body that is carrying on constructive and destructive metabolism could remain for a moment perfectly homogeneous or constant in chemical composition. Their bodies were not limited by a rigid envelope or capsule. Reproduction was affected by binary fission, the body dividing into two with a dumbbell-shaped figure. Their mode of life was vegetative, that is to say, they reacted upon their environmental medium by means of ferments secreted by their own body-substance. The earliest forms must have possessed the power of building up their protein-molecules from the simplest inorganic compounds; but different types of biococci, characterised each by specific reactions and idiosyncrasies, must have become differentiated very rapidly in the process of evolution and adaptation to divergent conditions of life.

Consideration of the existing types and forms of living organisms shows that from the primitive biococcal type the evolution of living things must have diverged in at least two principal directions. Two new types of organisms arose, one of which continued to specialise further in the vegetative mode of life, in all its innumerable variations, characteristic of the biococci, while the other type developed an entirely new habit of life, namely a predatory existence. I will consider these two types separately.

(1) In the vegetative type the first step was that the body became surrounded by a rigid envelope. Thus came into existence the bacterial type of organism, the simplest form of which would be a Micrococcus, a minute globule of chromatin surrounded by a firm envelope. From this familiar type an infinity of forms arise by processes of divergent evolution and adaptation. With increase in size of the body the number of chromatin-grains within the envelope increase in number, and are then seen to be imbedded in a ground-substance which is similar to cytoplasm, apparently, and may contain non-chromatinic enclosures. With still further increase of size the chromatin-grains also increase in number and may take on various types of arrangement in clumps, spherical masses, rodlets, filaments straight or twisted in various ways, or even irregular strands and networks,[22] and the cytoplasmic matrix, if it is

[22] See especially Dobell, 'Contributions to the Cytology of the Bacteria,' *Quart. Journ. Micr. Science*, lvi. (1911), pp. 461, 462. I cannot follow Dobell in applying the term 'nuclei' to these various arrangements of the chromatin-

correct to call it so, becomes correspondingly increased in quantity. I will not attempt, however, to follow up the evolution of the bacterial type further, nor to discuss what other types of living organisms may be affiliated with it, as I have no claims to an expert knowledge of these organisms. I prefer to leave to competent bacteriologists and botanists the problem of the relationships and phylogeny of the Cyanophyceæ, Spirochætes, &c., which have been regarded as having affinities with Bacteria.

(2) In the evolution from the biococcus of the predatory type of organism, the data at our disposal appear to me to indicate very clearly the nature of the changes that took place, as well as the final result of these changes, but leave us in the dark with regard to some of the actual details of the process. The chief event was the formation, round the biococci of an enveloping matrix of protoplasm for which the term periplasm (Lankester) is most suitable. The periplasm was an extension of the living substance which was distinct in its constitution and properties from the original chromatinic substance of the biococcus. The newly-formed matrix was probably from the first a semi-fluid substance of alveolar structure and possessed two important capabilities as the result of its physical structure; it could perform streaming movements of various kinds, more especially amœboid movement; and it was able to form vacuoles internally. The final result of these changes was a new type of organism which, compared with the original biococci, was of considerable size, and consisted of a droplet of alveolar, amœboid periplasm in which were imbedded a number of biococci. Whether this periplasm made its first appearance around single individual biococci, or whether it was from the first associated with the formation of zooglœa-like colonies of biococci, must be left an open question.

Thus arose in the beginning the brand of Cain, the prototype of the animal, that is to say, a class of organism, which was no longer able to build up its substance from inorganic materials in the former peaceful manner, but which nourished itself by capturing, devouring, and digesting other living organisms. The streaming movements of the periplasm enabled it to flow round and engulph other creatures; the vacuole-formation in the periplasm enabled it to digest and absorb the substance of its prey by the help of ferments secreted by the biococci. By means of these ferments the ingested organisms were killed and utilised as food, their substance being first broken down into simpler chemical constituents and then built up again into the protein-substances composing the body of the captor.

A stage of evolution is now reached which I propose to call the pseudomoneral or cytodal stage, since the place of these organisms in the general evolution of life corresponds very nearly to Haeckel's conception of the Monera as a stage in the evolution of organisms, though not at all to his notions with regard to their composition and structure. The bodies of these organisms did not consist of a homogeneous albuminous 'plasson,' but of a periplasm corresponding to the cytoplasm of the cell, containing a number of biococci or chromatin-grains. Thus their composition corresponded more clearly to that of plasson as conceived by Van Beneden, when he wrote: 'Si un noyau vient à disparaître dans une cellule, si la cellule redevient un cytode, les éléments chimiques du noyau et du nucléole s'étant repandus dans le protoplasme, le plasson se trouve de nouveau constitué.' If we delete from this sentence the word 'chimiques' and also the words 'et du nucléole,' and substitute for the notion of the chemical solution of the chromatin-substance that of scattered chromatin-grains in the periplasm, we have the picture of the cytodal stage of evolution such as I have imagined it. It should be borne in mind that the ultimate granules of chromatin are probably in many cases ultra-microscopic; consequently they might appear to be dissolved in this cytoplasm when really existing as discrete particles.

In the life-cycles of Protozoa, especially of Rhizopods, it is not at all infrequent to find developmental phases which reproduce exactly the picture of the

grains in Bacteria. Vejdovský compares them with chromosomes; but there is no evidence that they play the part in the division and distribution of the chromatin-grains which is the special function of chromosomes, as will be discussed in more detail presently.

pseudo-moneral stage of evolution, phases in which the nucleus or nuclei have disappeared, having broken up into a number of chromatin-grains or chromidia scattered through the cytoplasm. We do not know as yet of any Protozoa, however, which remain permanently in the cytodal stage, that is to say, in which the chromatin-grains remain permanently in the scattered chromidial condition, without ever being concentrated and organised into true nuclei; but it is quite possible that some of the primitive organisms known as Proteomyxa will be found to exhibit this condition and to represent persistent Pseudo-monera or cytodes.

The next stage in evolution was the organisation of the chromatin-grains (biococci) into a definite cell-nucleus. This is a process which can be observed actually taking place in many Protozoa in which 'secondary' nuclei arise from chromidia. It seems not unreasonable to suppose that a detailed study of the manner in which secondary nuclei are formed in Protozoa will furnish us with a picture, or rather series of pictures, of the method in which the cell-nucleus arose in phylogeny. To judge from the data supplied by actual observation, the evolution of the nucleus, though uniform in principle, was sufficiently diversified in the details of the process. As one extreme we have the formation of a dense clump of small, separate chromatin-grains, producing a granular nucleus of the type seen in Dinoflagellates, in Hæmogregarines, and in Diatoms. Amongst the chromatin-grains there may be present also one or more grains or masses of plastin forming true nucleoli. At the opposite extreme a clump of chromatin-grains becomes firmly welded together into a single mass in which the individual grains can no longer be distinguished, forming a so-called karyosome, consisting of a basis of plastin cementing or imbedding the chromatin-grains into a mass of homogeneous appearance. Whatever the type of nucleus formed, the concentration of the chromidia into nuclei does not necessarily involve all the chromidia, many of which may remain free in the cytoplasm.

In the chromidial condition the chromatin-grains scattered in the cytoplasm are lodged at the nodes of the alveolar framework.[23] Consequently a supporting framework of cytoplasmic origin, the foundation of the linin-framework, was probably a primary constituent of the cell-nucleus from the first. In many nuclei of the karyosomatic type it is very difficult to make out anything of the nature of a framework, which, however, in other cases is seen clearly as delicate strands radiating from the karyosome to the wall of the vacuole in which the karyosome is suspended. Probably such a framework is present in all cases, and each supporting strand is to be interpreted as the optical section of the partition between two protoplasmic alveoli.

With the formation of the nucleus the cytode or pseudo-moneral stage has become a true cell of the simplest type, for which I propose the term *protocyte*. It is now the starting-point of an infinite series of further complications and elaborations in many directions. It is clearly impossible that I should do more than attempt to indicate in the most summary manner the various modifications of the cell-type of organism, since to deal with them conscientiously would require a treatise rather than an address, and, moreover, many such treatises exist already. The most conspicuous modifications of cell-structure are those affecting the periplasm, or, as we may now term it, the cytoplasm. In the first place, the cell as a whole takes various forms; primitively a little naked mass of protoplasm tending to assume a spherical form under the action of surface-tension when at rest, the cell-body may acquire the most diverse specific forms maintained either by the production of envelopes or various kinds of exoskeletal formations on the exterior of the protoplasmic body, or of supporting endo-skeletal structures formed in the interior. The simple amœboid streaming movements become highly modified in various ways or replaced by special loco-motor mechanisms or organs, flagella, cilia, &c., of various kinds. The internal alveolar cytoplasm develops fibrillæ and other structures of the most varied nature and function, contractile, skeletal, nervous, and so forth. The vacuole-

[23] Cf. Dobell, 'Observations on the Life-History of Cienkowski's *Arachnula*,' *Arch. Protistenkunde*, xxxi. (1913), p. 322. The author finds that in *Arachnula* each nucleus arises from a single chromatin-grain, which grows to form a vesi-cular nucleus. Since the fully-formed nucleus contains numerous grains of chromatin, the original chromidiosome must multiply in this process.

system may be amplified and differentiated in various ways and the cytoplasm acquires manifold powers of internal or external secretion. And finally the cytoplasm contains enclosures of the most varied kind, some of them metaplastic products of the anabolic or catabolic activity essential to the maintenance of life, others of the nature of special cell-organs performing definite functions, such as centrosomes, plastids, chromatophores, &c., of various kinds.

With all the diverse modifications of the cytoplasmic cell-body the nucleus remains comparatively uniform. It may indeed vary infinitely in details of structure, but in principle it remains a concentration or aggregation of numerous grains of chromatin supported on some sort of framework over which the grains are scattered or clumped in various ways, supplemented usually by plastin or nucleolar substance either as a cementing ground-substance or as discrete grains, and the whole marked off sharply from the surrounding cytoplasm, with or without a definite limiting membrane. There is, however, one point in which the nucleus exhibits a progressive evolution of the most important kind. I refer to the gradual elaboration and perfection of the reproductive mechanism, the process whereby, when the cell reproduces itself by fission, the chromatin-elements are distributed between the two daughter-cells.

The chromatin-constituents of the cell are regarded, on the view maintained here, as a number of minute granules, each representing a primitive independent living individual or biococcus. To each such granule must be attributed the fundamental properties of living organisms in general; in the first place metabolism, expressed in continual molecular change, in assimilation and in growth, with consequent reproduction; in the second place specific individuality. As the result of the first of these properties the chromatin-granules, often perhaps ultra-microscopic, may be larger or smaller at different times, and they multiply by dividing each into two daughter-granules. As a result of the second property, chromatin-granules in one and the same cell may exhibit qualitative differences and may diverge widely from one another in their reactions and effects on the vital activities of the cell. The chromatin-granules may be either in the form of scattered chromidia or lodged in a definite nucleus. When in the former condition, I have proposed the term chromidiosome[24] for the ultimate chromatinic individual unit; on the other hand, the term chromiole is commonly in use for the minute chromatin-grains of the nucleus. The terms chromidiosome and chromiole distinguish merely between the situation in the cell, extranuclear or intranuclear, of the individual chromatin-grain or biococcus.

In the phase of evolution that I have termed the pseudomoneral or cytodal phase, in which the organism was a droplet of periplasm containing scattered biococci or chromidiosomes, metabolism would result in an increase in the size of the cytode-body as a whole, accompanied by multiplication of the chromidiosomes. Individualisation of the cytodes would tend to the acquisition of a specific size, that is to say, to a limitation of the growth, with the result that when certain maximum dimensions were attained the whole cytode would divide into two or more smaller masses amongst which the chromidiosomes would be partitioned.

In the next stage of evolution, the protocyte with a definite nucleus, it is highly probable that at each division of the cell-body, whether into two or more parts, the primitive method of division of the nucleus was that which I have termed elsewhere 'chromidial fragmentation';[25] that is to say, the nucleus broke up and became resolved into a clump of chromidiosomes, which separated into daughter-clumps from which the daughter-nuclei were reconstituted. Instances of nuclear divisions by chromidial fragmentation are of common occurrence among the Protozoa and represent probably the most primitive and direct mode of nuclear division.

It is clear, however, that if the chromatin-grains are to be credited with specific individuality and qualitative differences amongst themselves, this method of nuclear division presents grave imperfections and disadvantages, since even the quantitative partition of the chromatin is inexact, while the qualitative partition is entirely fortuitous. Chromidiosomes having certain

[24] *Introduction of the Study of the Protozoa*, Arnold, 1912, p. 65.
[25] *Op. cit.* p. 101.

specific properties might all become accumulated in one daughter-cell, and those having opposite properties in the other, so that the two daughter-cells would then differ entirely in their properties.

I can but refer briefly here in passing to the interesting theory put forward by Butschli, to the effect that sexual phenomena owe their first origin to differences between cellular organisms resulting from the imperfections of the primitive methods of cell-division. If we assume, for instance, as so many have done, that one of the earliest qualitative differences between different chromatin-granules was that while some influenced more especially the trophic activities of the cell, others were concerned specially with kinetic functions; then it might easily happen, after nuclear division by chromidial fragmentation, that all, or the majority of, the kinetic elements pass into one of the two daughter-cells, while its twin-sister obtains an undue preponderance of trophic chromatin. As a consequence, some cells would show strong kinetic but feeble trophic energies and others the opposite condition, and in either case the viability of the cells would be considerably impaired, perhaps inhibited. If it be further assumed that cells of opposite tendencies, kinetic and trophic, attract one another, it is easy to see that the union and fusion of two such cells, the one unduly kinetic (male) in character, the other with a corresponding trophic (female) bias, would restore equilibrium and produce a normal cell with kinetic and trophic functions equally balanced. On this view, sexual union, at its first appearance, was a natural remedy for the disadvantages arising from imperfect methods of nuclear division.

It is not surprising, therefore, to find that the process of nuclear division undergoes a progressive elaboration of mechanism which has the result of ensuring that the twin sister-granules of chromatin produced by division of a single granule shall be distributed between the two daughter-cells, so that for every chromatin-grain obtained by one daughter-cell an exact counterpart is obtained by the other; in other words, of ensuring an exact qualitative, as well as quantitative, partition of the chromatin-particles. In its perfect form this type of nuclear division is known as karyokinesis or mitosis, and all stages in its progressive development are to be found in the Protozoa.

In the evolution of nuclear division by karyokinesis two distinct processes are being developed and perfected in a parallel manner, but more or less independently; first, the method of the partition and distribution of the chromatin-grains between the two daughter-nuclei; secondly, the mechanism whereby the actual division of the nucleus and the separation of the two daughter-nuclei are effected in the cell-division. I have dealt elsewhere [26] with the evolution of the mechanism of karyokinesis as exemplified by the numerous and varied types of the process found amongst the Protozoa, and I need not discuss the matter further here, but the behaviour of the chromatin-grains may be dealt with briefly. The main feature in the process of the exact quantitative and qualitative distribution of the daughter-chromatin between the daughter-nuclei is the aggregation of the chromatin-grains or chromioles into definite, highly individualised structures known as chromosomes. In the most perfected forms of the process of chromosome-formation the chromioles become united into a linear series termed by Vejdovský a chromoneme, which is supported upon a non-chromatinic basis or axis. According to Vejdovský, the supporting substance consists of linin; R. Hertwig, however, in his well-known studies on *Actinosphærium* [27] considers that the supporting and cementing substance of the chromosome is plastin derived from the substance of the nucleoli. However that may be, the essential feature of the chromosome is the cementing together of the chromioles to form the chromoneme, a thread of chromatin which may be disposed in various ways on the supporting axis, sometimes being wound spirally round it (Vejdovský).

The actual division of the chromatin takes place by the longitudinal splitting of the chromoneme, in other words, by simultaneous division into two of each of the chromioles of which the thread is composed. In this way every chromiole which was contained in the original chromoneme is represented by a daughter-chromiole in each of the two daughter-chromonemes. It follows

[26] *Op. cit.* pp. 105-120.

[27] *Abhandl. bayer. Akad.* (II. Cl.) xix. 1898.

that the familiar process of the splitting of the chromosomes in karyokinesis is a mechanism which brings about in the most simple, sure and direct manner an exact quantitative and qualitative partition of the chromatin-grains between the two daughter-nuclei. In the sequel each daughter-nucleus is built up, according to Vejdovský, entirely and solely from one of the two daughter-clumps of chromosomes, and each chromosome is resolved again into its constituent chromioles, giving rise in some cases to a definite portion of the nucleus, a karyomere, from which again, at the next nuclear division, the chromosome is reconstituted by the chromioles falling into line in an orderly manner.

The chromatin-cycle of a cell in which the process of division by karyokinesis takes place in its most perfectly developed form, may, therefore, be conceived as follows. The nucleus in its resting state contains a definite number of companies or brigades of chromatinic units (chromioles), each brigade spread over a certain extent of the nuclear framework forming a karyomere. As a preparation to division each separate brigade of chromioles falls into line as the chromoneme, forming with its supporting substance the chromosome; there are formed, therefore, just so many chromosomes as there were karyomeres in the nucleus. In this disciplined and orderly array each chromiole undergoes its division into two daughter-chromioles, so that each file or chromoneme of chromioles splits into two files. At the reconstitution of the daughter-nuclei each daughter-chromosome gives rise to a karyomere again, the chromioles falling out of the ranks and disposing themselves in an apparently irregular manner on the newly-built framework of the daughter-nucleus to constitute their own particular karyomere. Thus karyokinesis differs only from the most primitive method of division by chromidial fragmentation in that what was originally a haphazard method of distribution has become a disciplined and orderly manœuvre, performed with the precision of the parade-ground, but in a space far less than that of a nutshell.

In the nuclear division of Protozoa, without going into excessive detail, it may be stated broadly that all stages are to be found of the gradual evolution of the tactical problem which constitutes karyokinesis. The chromosomes in the more primitive types of nuclear division are usually very numerous, small, irregular in number and variable in size; the splitting of the chromosomes is often irregular and not always definitely longitudinal; and distinct karyomeres have not so far been recognised in the nuclei of Protozoa. In many cases only a part, if any, of the chromatin falls in to form the chromosomes, and a greater or less amount of it remains in the karyosome, which divides directly into two. The various types of nuclear division in Protozoa have been classified as promitosis, mesomitosis and metamitosis, for detailed accounts of which those interested must refer to the text-books and original descriptions.

I have dealt briefly with the problem of the evolution of karyokinesis because the process of nuclear division is, in my opinion, of enormous importance in the general evolution of living organisms. I have expressed elsewhere [28] the opinion that the very existence of multicellular organisms composed of definite tissues is impossible until the process of karyokinesis has been established and perfected. For tissue-formation it is essential that all the cells which build up any given tissue should be similar, practically to the point of identity, in their qualities; and if it is the chromatin-elements of the cell which determine its qualities and behaviour, then the exact qualitative division of the chromatin, as effected in karyokinesis is indispensable as a preliminary to the production of identically-similar daughter-cells by division of a parent-cell. Hence it becomes intelligible why, amongst Metazoa, we find the occurrence of nuclear division by karyokinesis in its most perfect form to be the rule, and 'direct' division of the nucleus to be the rare exception, while on the other hand, in the Protista, and especially in the Protozoa, we find every possible stage in the gradual evolution of the exact partition of the chromatin in the process of nuclear division, from chromidial fragmentation or the most typical amitosis up to processes of karyokinesis as perfect as those of the Metazoa.

[28] *Op. cit.* p. 120.

There now remains only one point of general interest in the evolution of the cell to which brief reference must be made, namely, the divergence of animal and vegetable cells. Not being a botanist, I desire to approach this question with all caution; but as a protozoologist it seems to me clearly indicated that the typical green plant-cell took origin amongst the Flagellata, in that some members of this group of Protozoa acquired the peculiar chromatophores which enabled them to abandon the holozoic or animal mode of life in exchange for a vegetative mode of nutrition by means of chlorophyll-corpuscles. It is well known that many of these creatures combine the possession of chlorophyll with an open, functional mouth and digestive vacuoles, and can live either in the manner of plants or of animals indifferently or as determined by circumstances. It would be interesting to know exactly what these chromatophores, at their first appearance, represent; whether they are true cell-organs, or whether, as some authorities have suggested, they originated as symbiotic intruding organisms, primitively independent. I do not feel competent to discuss this problem. I would only remark here, that if the green plant-cell first arose amongst the Flagellata, then the distinction between plant and animal (that is, green plant and animal) is not so fundamental a divergence in the series of living beings as is popularly supposed, but is one which did not come into being until the evolution of organisms had reached a relatively advanced stage, that, namely, of the true nucleated cell.

I have confined myself in this address to the evolution of the cell as this organism is seen in its typical form in the bodies of the multicellular organisms, starting from the simplest conceivable type of living being, so far as present knowledge enables us to conceive it. But there is not the slightest reason to suppose that the evolution of the Protista took place only in the direction of the typical cell of the cytologist. Besides the main current leading up to the typical cell there were certainly other currents tending in other directions and leading to types of structure very unlike the cells composing the bodies of multicellular organisms. It is impossible that I should do more here than indicate some of the divergent lines of evolution, and I will confine myself to those seen in the Protozoa.

Taking as the starting-point and simplest condition in the Protozoa a simple cell or protocyte, in which the body consists of a small mass of cytoplasm containing a nucleus, with or without chromidia in addition, an early specialisation of this must have been what I may term the plasmodial condition, typical of Rhizopods, in which the cytoplasm increased enormously to form relatively large masses. The nucleus meanwhile either remains single and grows very large or, more usually, a great number of nuclei of moderate size are formed. From this large plasmodial type is to be derived the foraminiferal type, characterised by the creeping habit of life, and probably also the radiolarian type, specialised for the floating pelagic habit. Both foraminiferal and radiolarian types are characterised by an excessive development and elaboration of skeletal structures, and the geological record proves that these two types of organisms attained to a high degree of specialisation and diversity of form and structure at a very early period.[29] The Mycetozoa exemplify another development of the creeping plasmodial type adapted to a semi-terrestrial mode of life.

In the Mastigophora the body generally remains small, while developing organs of locomotion and food-capture in the form of the characteristic flagella. In this class there is a strong tendency to colony-formation brought about by incomplete separation of sister-individuals produced in the ordinary process of reproduction by binary fission. The so-called colonies (they would better be termed families) show a most significant tendency to individualisation, often accompanied by physiological and morphological specialisation of the component flagellate individuals.

As an offshoot, probably, from ancestors of the Mastigophoran type arose the Infusoria, the Ciliata and their allies, representing by far the most highly-organised unicellular type of living being. No cell in the bodies of the Metazoa attains to such a complication of structure as that exhibited by many

[29] For Foraminifera see especially Heron-Allen, *Phil. Trans.* (B), vol. 206 (1915), p. 229.

Ciliates. In the Metazoa the individual cells may be highly specialised for some particular function of life; but a Ciliate is a complete and independent organism and is specialised for each and all of the vital functions performed by the Metazoan body as a whole. From the physiological standpoint a Ciliate (or any other Protist) is equivalent and analogous to a complete Metazoon, say a man, but I cannot for a moment agree with Dobell [30] that the body of a Ciliate is homologous with that of a Metazoon—not at least if the word homologous be used in its usual biological sense of homogenetic as opposed to homoplastic. Dobell appears to me to negative his own conclusion when he maintains that the body of a Ciliate is 'non-cellular' while admitting that the Metazoon is multicellular; how then can they be said to be homologous? Only if the term homologous be used in a sense quite different from its ordinary significance.[31]

In addition to the highly-developed structural differentiation of the body the Infusoria exhibit the extreme of specialisation of the nuclear apparatus in that they possess, as a rule, two distinct kinds of nuclei, micronuclei and macronuclei, composed respectively of generative and trophic chromatin, as already pointed out. This feature is, however, but the culminating point in a process of functional specialisation of the chromatin which can be observed in many Protozoa of other classes, and which, moreover, is not found invariably in its complete form in all Ciliata.

In this address I have set forth my conceptions of the nature of the simplest forms of life and of the course taken by the earliest stages of evolution, striving all through to treat the problem from a strictly objective standpoint, and avoiding as far as possible the purely speculative and metaphysical questions which beset like pitfalls the path of those who attack the problem of life and vitalism. I have, therefore, refrained as far as possible from discussing such indefinable abstractions as 'living substance' or 'life,' phrases to which no clear meaning can be attached.

How far my personal ideas may correspond to objective truth I could not, of course, pretend to judge. It may be that the mental pictures which I have attempted to draw are to be assigned, on the most charitable interpretation, to the realm of poetry, as defined by the greatest of poets, rather than of science.

'The lunatic, the lover and the poet
Are of imagination all compact;

. . . .

And as imagination bodies forth
The forms of things unknown, the poet's pen
Turns them to shapes and gives to airy nothings
A local habitation and a name.'

If I might be permitted to attempt an impartial criticism of my own scheme, I think it might be claimed that the various forms and types of organisms in my evolutionary series, namely, the simple cell or protocyte, the cytode or pseudomoneral stage, the micrococcus, even the biococcus, are founded on concrete evidence and can be regarded as types actually existent in the present or past. On the other hand the *rôle* assigned by me to each type in the pageant of evolution is naturally open to dispute. For example, I agree with those who derive the Bacteria as primitive, truly non-cellular organisms, directly from the biococcus through an ancestral form, and not at all with those who would regard the Bacteria as degenerate or highly-specialised cells. But the crux of my scheme is the homology postulated between the biococcus and the chromatinic particle—chromidiosome or chromiole—of true cells. In support of this view, of which I am not the originator, I have set forth the reasons which have convinced me that the extraordinary powers and activities exhibited by the chromatin in ordinary cells are such as can only be explained on the hypothesis that the ultimate chromatinic units are to be regarded as independent living beings, as much so as the cells composing the bodies of multicellular organisms; and, so far as I am concerned, I must leave the matter to the judgment of my fellow-biologists.

[30] *Journal of Genetics*, iv. (1914), p. 136.
[31] See Appendix A.

I may point out in conclusion that general discussions of this kind may be useful in other ways than as attempts to discover truth or as a striving towards a verity which is indefinable and perhaps unattainable. Even if my scheme of evolution be but a midsummer-night's fantasy, I claim for it that it co-ordinates a number of isolated and scattered phenomena into an orderly, and, I think, intelligible sequence, and exhibits them in a relationship which at least enables the mind to obtain a perspective and comprehensive view of them. Rival theories will be more, or less, useful than mine, according as they succeed in correlating more, or fewer, of the accumulated data of experience. If in this address I succeed in arousing interest and reflection, and in stimulating inquiry and controversy, it will have fulfilled its purpose.

Appendix A.—*The Cell-Theory.*

The most recent attack on the Cell-theory, as it is understood by the majority of modern biologists, has been made by Mr. Dobell, who, if I understand him rightly, refuses to admit any homology between the individual Protistan organism and a single cell of the many that build up the body of a Metazoon. On the contrary, he insists that the Protist is to be regarded as homologous with the Metazoan individual as a whole. On these grounds he objects to Protista being termed 'unicellular' and insists that the term 'non-cellular' should be applied to them.

As regards the cellular nature of the Protista, it is one of my aims in this address to show that amongst the Protista all stages of the evolution of the cell are to be found, from primitive forms in which the body cannot be termed a cell without depriving the term 'cell' of all definable meaning, up to forms of complex structure in which all the characteristic features of a true cell are fully developed. Thus in the Protozoa we find the protoplasmic body differentiated into nucleus and cytoplasm; the nucleus in many cases with a structure comparable in every detail to that of the nucleus of an ordinary body-cell in the Metazoa; reproduction taking place by division of the body after a karyokinetic nuclear division often quite as complicated as that seen in the cells of the Metazoa and entirely similar both in method and in detail; and in the sexual process a differentiation of the gametes on lines precisely similar to those universal in Metazoa, often just as pronounced, and preceded also in a great many cases by phenomena of chromatin-reduction comparable in principle, and even sometimes in detail, with the reduction-processes occurring in Metazoa. I really feel at a loss to conceive what further criteria of homology between a Protozoon and a Metazoan cell could be demanded by even the most captious critic. On the ground of these and many other similarities in structure and behaviour between the entire organism in the Protozoa and the individual cell, whether tissue-cell or germ-cell, in the Metazoa, the case seems to me overwhelmingly convincing for regarding them as truly—that is to say, genetically—homologous.

Looking at the matter from another point of view, namely, from the stand-point of the Metazoa, it is true that in the groups of most complicated and highly organised structure the cells often develop secondary connections or fusions due to incomplete division, to such an extent that in parts of the body the individuality of the primitively distinct cells may be indicated only by the nuclei (as may occur also in Protozoa, for example, in associated gregarines); but in all Metazoa certain of the cells retain permanently their complete independence and freedom of movement and action. In the Metazoa possessing the simplest and most primitive types or organisation, such as sponges and cœlenterates, the cells composing the body show far greater independence of action, and in the course of ontogeny entire groups of cells may alter their relative positions in the body as the result of migrations performed by individual cells; while it is now well known that if the adult sponge or hydroid be broken up completely into its constituent cells, those cells can come together again and build up, by their own individual activity, the regenerated body of the organism. For these reasons it seems to me impossible to regard the body-cells of the Metazoa otherwise than as individual organisms complete in themselves, primitively as independent as the individual Protozoon, and in every way comparable to it.

From the considerations summarised very briefly in the two foregoing para-

graphs and capable of much greater amplification and elaboration, the view generally held that the entire organism of a Protozoon is truly homologous with a single body-cell of a Metazoon seems to me quite unassailable, and to have gained in force greatly from recent investigations both upon Protozoa and Metazoa. On the other hand, any Protist, as an organism physiologically complete in itself, is clearly analogous to the entire individual in the Metazoa —a comparison, however, which leaves the question of genetic homology quite untouched.

As regards the application of the term unicellular or non-cellular to the Protozoa, it is evident that if the evolution of living beings had never proceeded beyond the stage of the Protista, and if no multicellular organisms had " could then never have been invented by an ntelligent being studying otner living beings, supposing for an instant the possibility of such intelligence existing apart from a mammalian brain. So long as the Protozoa are studied entirely by themselves, without reference to any other forms of life, they may be termed non-cellular in the sense that they are not composed of cells. It is only when they are compared with multicellular organisms that the term unicellular becomes applicable on the ground of the homology already discussed between the Protozoon and the body-cell of the Metazoon.

British Association for the Advancement of Science.

SECTION E: MANCHESTER, 1915.

ADDRESS

TO THE

GEOGRAPHICAL SECTION

BY

MAJOR H. G. LYONS, D.Sc., F.R.S.,

PRESIDENT OF THE SECTION.

The Importance of Geographical Research.

THIS year, when the British Association is holding its meeting in times of the utmost gravity, the changed conditions which have been brought about by this War must occupy the attention of all the Sections to a greater or less extent, and our attention is being called to many fields in which our activities have been less marked or more restricted than they might have been, and where more serious study is to be desired. The same introspection may be usefully exercised in geography, for although that branch of knowledge has undoubtedly advanced in a remarkable degree during the last few decades, we have certainly allowed some parts of the subject to receive inadequate attention as compared with others, and the necessity for more serious study of many of its problems is abundantly evident.

Nor is the present occasion ill adapted to such an examination of our position, for when the British Association last met in this city, now twenty-eight years ago, the President of this Section, General Sir Charles Warren, urged in his address the importance of a full recognition of geography in education on the grounds that a thorough knowledge of it is required in every branch of life, and is nowhere more important than in diplomacy, politics, and administration.

Matters have certainly advanced greatly since that time, and a much fuller appreciation of geography now exists than that which formerly prevailed. At the time of the address to which I have referred the serious study of geography in this country was on the eve of important developments. The Council of the Royal Geographical Society had for some time been urging the importance of geography being studied at the Universities so that there should be an opportunity for advanced students to qualify themselves as scientific geographers by study and original research in the subject. The time had arrived for this ideal to become an accomplished fact, and in the following year, 1888, a Reader in Geography was appointed at Oxford University, and a Lectureship in the same subject was established at Cambridge. Since then the advance has been steady and continuous not only in the increased attention given to the subject, but also in the way in which it is treated. The earlier bald and unattractive statistical presentation of the subject has now been almost everywhere replaced by a more intelligent treatment of it, in which the influences of the various environments upon the life which inhabits a region are appreciated, and the responses to such influences are followed up. Instruction in the subject is given by those who have seriously studied it, who realise its importance, and who are in a position to train up new scientific workers in the field of geography. Though much remains to be done there should be now a steady output of geographical investigators capable of providing an ever-increasing supply of

carefully observed data, which they will have classified methodically and discussed critically, in order that these may be utilised to form sound generalisations as to their relationships and sequence in accordance with the method which is employed in all scientific work.

In order that we may see what advance has been made in the scientific study of geography in this country during the last quarter of a century, we must turn to the results that have been attained by the activity of geographical investigators who have devoted themselves to the serious study of various phenomena, and the detailed investigation of particular regions. If we do so I think that we must admit that the number of original investigators in scientific geography who are extending its scope in this way is not so large as it might be, nor are we yet utilising sufficiently all the material which is available to us. Anyone who will examine the geographical material which has been published in any period which he may select for review will find that purely descriptive treatment still far outweighs the analytical treatment which alone can lead to definite advances in scientific geography. If pleasing descriptions of this or that locality are sought for, they are for the most part to be found readily in the very large amount of such material that has been and is being published each year by residents, travellers, and explorers; but if information is desired in the prosecution of a piece of geographical research, we are checked by the lack of precise details. Few of this class of descriptions are sufficiently definite to enable the necessary comparisons to be made between one locality and others which are similarly situated; thoroughly quantitative treatment is for the most part lacking, and while a pleasing picture is drawn which is probably true in character, it is usually inadequately furnished with those definite facts which the geographer requires.

I propose, therefore, to examine a little more closely the question of geographical investigation and research in order to see where we stand and in what direction it behoves us to put forth our energies to the end that a branch of knowledge which is of such importance shall rest upon that basis of detailed study and investigation which alone can supply the starting-point for further advance. The intricate and complicated character of the subject, the extent of its purview, the numerous points at which it touches and imperceptibly passes into other well-defined branches of knowledge, render the study of geography very liable to degenerate into a purely descriptive treatment of the earth's surface and all that is to be found thereon, rather than to follow the narrow path of scientific progress in which the careful collection of data furnishes the material for systematic discussion and study in order that trustworthy generalisations may be reached.

The opportunity to undertake long journeys through distant lands comes to few of us, but this is not the only direction in which research can be profitably undertaken, for there is no part of these islands where a geographer cannot find within his reach some geographical problem which is well worth working out, and which, if well and thoroughly done, will be a valuable contribution to his science. Even for such as cannot undertake such field work the library will provide a host of subjects which have not received nearly the amount of attention and of careful study that they deserve. The one thing essential is that the study should be as thorough as possible, so that all the contributory lines of evidence shall be brought together and compared, and so that the result may prove to be a real addition to geographical science on which other workers may in their turn build.

For those who desire to undertake such investigations there is at any rate no lack of geographical material, for travellers, explorers, and others engaged in various occupations in every part of the world are continually recording their experiences and describing their surroundings in books and pamphlets; they recount their experiences to the Geographical Societies, who apparently have no difficulty in obtaining communications of wide interest for their meetings. Most portions of the British Empire as well as regions belonging to other nations are in these days more or less fully examined, surveyed, and investigated with a view to their development, and those who undertake such work have ample opportunities for the most part for preparing descriptions of the lands in which they have sojourned and with which they are well acquainted. But although the material is so ample the quality of it is not

generally such as makes it suitable for an adequate study of the phenomena or the region to which it relates. The ease with which a tract of country or a route can be described by the traveller, and the attractiveness of such a description of a little-known region, results in the provision of a vast quantity of geographical information, the greater part of which has probably been collected by those who have no adequate training in the subject. In such cases it is not uncommon for the writer to disclaim any geological or botanical knowledge, for instance, but the great majority of those to whom the opportunity is given to travel and see new lands and peoples are fully convinced of their competence to describe accurately and sufficiently the geography of the regions which they traverse. But anyone who has had occasion to make use of such material in a serious investigation is only too well aware how little precise and definite information he will be able to extract from the greater part of this wealth of material, and in most cases this is due to the traveller's lack of geographical knowledge. He probably does not know the phenomena which should be observed in the type of region which he is traversing, nor can he read the geographical evidence which lies patent to a trained observer at every point of the journey; much, therefore, of what he records may be of interest, but probably lacks data which are essential to the geographer if he is to understand the geographical character of the region, and utilise it properly.

Thus it happens that although the amount of geographical material which is being garnered may be large, the proportion of it which is available for use in a scientific investigation of an area is smaller than is probably realised by those who have not made the experiment. And yet it is only by this scientific investigation of selected localities or of a single phenomenon and by working them out as thoroughly as possible that any real advance in geographical science can be made. The accounts of such pieces of work will not appeal to those who desire picturesque descriptions of little-known lands, but they will be welcomed by geographers who can appreciate the value of such studies. There should now be an ever-increasing number of such geographers, trained to proceed in their investigations by the true scientific method, and there should be a very considerable amount of sound work in various branches of the subject which aims at thoroughly investigating some phenomenon, or group of phenomena, so as to present a grouping of data, carefully verified and critically discussed, in order to arrive at conclusions which may form a useful addition, however small, to the sum of our geographical knowledge.

So far as I am able to judge, the output of serious work of this character is not nearly as large as it should be, and I would indicate some fields in which there is a lack of individual work of this character. Until more of it is undertaken we shall lack in this country the material from which the foundations of scientific geography can be built up, and while our own islands and the various parts of the British Empire furnish unrivalled opportunities for such work, there are still far too many subjects where the most thorough investigations have been made in other countries.

Mathematical Geography presents a field for research which had comparatively little attention paid to it in this country. In many respects this part of the subject is peculiarly suitable for such treatment, since it admits of the employment of precise methods to an extent which is not always practicable in cases where so many of the factors can only be approximately defined. The determination of positions on the earth's surface is carried to great refinement in the national surveys of most civilised countries in order to furnish the necessary controls for the preparation of large-scale maps, but when we pass to the location of travellers' routes, where considerable allowance has to be made for the conditions under which the observations have to be taken, we find that very inadequate attention is usually paid to the discussion of the results. Usually a mean value for each latitude, longitude or azimuth is obtained by the computer, and he remains satisfied with this, so that when the route of another traveller follows the same line or crosses it at one or more points, it is almost impossible for the cartographer to say which of the two determinations of any position is entitled to the greater confidence. In this class of work, whether the results are obtained from absolute observations at certain points or from the direction of march, and the distance traversed, it is quite practicable to determine the range of uncertainty within which the

positions of different points are laid down, and it is eminently desirable that this should always be done in order that the adjustment of various routes which may intersect in partially-known regions may be adjusted in accordance with definite mathematical processes. Some important expeditions on which infinite labour and considerable sums have been expended have presented their results, in so far as they relate to the routes which have been followed and the position of points which have been determined, in such a way that it is impossible to say within what precision such positions have been determined, and consequently any combination of these results with those of later expeditions has to be carried out empirically, since adequate data are no longer available for the employment of better and more scientific methods.

This crude and unsatisfactory way of treating observations, which in many cases have been obtained under conditions of the greatest difficulty and even hardship, is largely due to the lack of interest which geographers have shown in this part of their subject. Methods of observation and methods of computation are rarely discussed before any of our Geographical Societies or in any of our publications, and it is only by such discussions that the importance of properly working out the available material at a time when the observer can be consulted on points which are doubtful, or where further explanation is desirable, becomes generally appreciated.

No set of physical or astronomical observations is ever discussed, or even presented without the degree of precision or reliability being definitely stated; yet in geography this sound rule is too often neglected.

There are several regions where travellers' routes intersect which should provide ample material for the careful reduction and adjustment of the results. I fear, however, that there would be great difficulty in obtaining the original observations which are indispensable in such an investigation, and in the interest of research it is highly desirable that the original documents of all work of importance should be preserved and the place where they may be consulted recorded in the published account.

There is room in the geographical investigation of sea and land, even within the limits of the British Empire, for the employment of methods of observation and computation of the highest precision as well as of the simpler and more approximate kinds, but everyone who presents the results of his work should deem it his first duty to state explicitly the methods which he employed, and the accuracy to which he attained, in such a form that all who make use of them can judge for themselves of the degree of their reliability.

In such work, while the instruments used are of great importance, too often the briefest description, such as 'a 4-inch theodolite,' is deemed sufficient. If the observer wishes his work to be treated seriously as a definite contribution to science we require to know more than this, and a clear account of the essentials of the instrument, a statement of its errors, and of the methods of observation adopted are the least that will suffice The account of any expedition should treat so fully of the instruments, observations, and computations utilised to determine the positions of places visited that anyone can re-examine the evidence and form his opinion on the value of the results obtained. A mere tabular statement of accepted values, which frequently is all that is provided, is of small value from a scientific point of view. Probably one reason for this state of things is that too little attention is being paid by geographers to their instruments. Theodolites, levels, compasses, clinometers, tacheometers, plane-tables, pantographs, co-ordinatographs, planimeters, and the many other instruments which are used by the surveyor, the cartographer, the computer, have in no case arrived at a final state of perfection, but it is seldom that we find a critical description of an instrument in our journals. Descriptions there are from time to time, but these are for the most part weak and insufficient. Not only is a technical description required, which treats fully of both the optical and mechanical details, but we need an extended series of observations with the instrument which have been made under the ordinary conditions of practical work, and these must be mathematically analysed, and the degree of the reliability of the results clearly demonstrated. The description should be equally thorough and complete, including scale drawings showing the construction of the instrument as well as photographs of it. Nothing less than this is of any use to the scientific cartographer.

While I am on the subject of instruments I would draw attention to the importance of the whole history of the development of surveying instruments. In the latter part of the eighteenth century Great Britain provided the best class of surveying instruments to all countries of Europe, at a time when high-class geodetic work was being commenced in several countries; and about this time von Reichenbach spent a part of his time in this country working in the workshops of Dollond and learning this particular class of work. Upon his return to Bavaria he set up at Munich that establishment which soon provided instruments of the highest class for many of the cadastral surveys which were being undertaken in Central Europe. At Munich there is now a fine typical collection of such instruments, but in this country the early advances of British instrument-makers of surveying instruments are far from being adequately represented in our National Museum in a manner commensurate with their importance. The keen and enlightened zeal of geographers who are interested in this branch of the subject would doubtless quickly bring to light much still remaining that is of great interest, but which is yet unrecognised, while a closer attention to instrumental equipment would lead to improvements and advances in the types that are now employed. There is no modern work in this country on the development of such instruments, and references to their history are conspicuously rare in our journals, so that there is here an opportunity for those whose duties prevent them from undertaking travel or exploration of a more ambitious kind. In the same way, those whose opportunities of field work are few can find a promising field of study in the early methods and practice of surveying which have been discussed by many authors from classical times onwards, and for which a considerable amount of material exists.

In Geodesy and Surveying of high precision there is ample scope for all who are attracted by the mathematical aspect of the subject; the critical discussion of the instruments and methods employed and results obtained, both in this country and in other lands, provides opportunity for much work of real value, while its bearing upon geology, seismology, &c., has not yet been adequately treated here. The detailed history of this part of our subject is to be found in papers which have been published in the technical and scientific journals of other countries for the most part; here too little attention has been given to the subject, in spite of the large amount of geodetic work which has been executed in the British Empire, and which remains to be done in our Colonies and overseas Dominions.

The final expression of the surveyor's detailed measurements is found in the map, and the adequate representation of any land surface on a map-sheet is both a science and an art. Here we require additional work on all sides, for there is hardly any branch of geography which offers so remunerative a field for activity as cartography. We need the co-operation of trained geographers to study requirements, and to make acquaintance with the limits of technical methods of reproduction, so that they may be in a position to deal with many questions which arise in the preparation of a map regarding the most suitable mode of presentation of data, a matter which is purely geographical, but which at the present time is too often left to the skilled draughtsman. Neither the compilation nor the reduction of maps are merely mechanical processes. The first requires great skill and care as well as technical knowledge and a sound method of treatment if the various pieces of work, which are brought together to make up the map of any considerable area, are to be utilised according to their true worth. This demands a competent knowledge of the work which has been previously done on the region, a first-hand acquaintance with the data collected by the earlier workers, and the critical examination of them in order that due weight may be given to the better material in the final result. This is not a task to be handed over to the draughtsman, who will mechanically incorporate the material as though it were all of equal accuracy, or will adjust discrepancies arbitrarily and not on any definite plan. Such preliminary preparation of cartographical material is a scientific operation which should be carried out by scientific methods and should be completed before the work reaches the draughtsman, who will then have but to introduce detail into a network of controls which has been prepared for him and of which the accuracy at all points has been definitely ascertained. Similarly in the second case the

elimination of detail which must of necessity be omitted is an operation needing the greatest skill, a full understanding of the material available, and an adequate appreciation of the result which is being aimed at, such as is only to be found in a competent geographer who has made himself intimately acquainted with all the material which is available and has his critical faculty fully developed.

The use of maps has steadily increased of recent years, but we should look forward to an even more widely extended use of them in the future; and this will be greatly facilitated if there are geographers who have made themselves masters of the technique of map reproduction and, as scientific geographers, are prepared to select such data as are needed for any particular class of map on a well-considered method, and not by the haphazard procedure to which the want of a scientific study of cartographic methods must inevitably lead. The paucity of papers dealing with practical cartography and the compilation of maps is clear proof that this branch of the subject awaits far more serious attention than it now receives.

All these problems are well within the reach of the geographer to whom the opportunity of travel in other regions does not come, and in them he will find ready to his hand a field of research which is well worth working and which will amply repay any labour that is spent upon it. The same precise methods of investigation which are employed in the discussion of observations should be applied to all cartographic material in order to ascertain the exact standard of its reliability, in which is included not only the correctness of distance and direction, but also the accuracy of the information which has been incorporated in it; and these may be brought to bear also on those early maps of which so many are preserved in our libraries in this country. In this field of study several investigators have already achieved results of great interest and value, but I think that they will be ready to admit that there is here a wide and profitable field of activity for many more workers who will study closely these early maps and, not being contented with verbal descriptions, will use quantitative methods wherever these are possible.

In the study of map projections some activity has been visible in recent years, and we may hope that those who have worked in this branch of the subject will see that British Geography is provided with a comprehensive manual of this subject which will be worthy of the vast importance of cartography to the Empire. The selection of suitable projections is receiving much more attention than was formerly accorded to it, but the number of communications on this subject which reach geographical journals are few and far between. The subject is not one which can appeal strongly to the amateur geographer, but its importance renders it imperative that the scientific geographer who realises its intimate bearing upon all his work should so arrange that the matter does not fall into the background on this account.

A closer relation and a more active co-operation between those who are prepared to work seriously at cartography and its various problems may reasonably be expected to raise the standard of that class of map which is used to illustrate books of travel, or works descriptive of a region. At the present time the inadequate character of many of the maps and plans which are reproduced in such publications shows clearly that the public demand for maps which have been compiled with a view to illustrating the volume in question is still very ineffective.

The whole subject of cartography, with its component parts of map projection, compilation, reproduction, cartometry and the history of its development, is so important, not only to the individual geographer but also to the advancement of scientific geography, that we should aim at fostering it and encouraging the study of it in every way, and it will be the zeal of individuals rather than the benevolent aid of institutions which will achieve this.

But it may be suggested that the lack of activity in Mathematical Geography is due to the somewhat specialised nature of the subject, and to the fact that the number of those who have received an adequate mathematical training and are prepared to devote themselves to geography is few. When we turn to Physical Geography in its treatment of the land we do find a field which has been more actively worked, for this is just the one to which the traveller's and

explorer's observations should contribute most largely, and where therefore their material should be utilised with the best results. Even here there is room for much more work of the detailed and critical type, which is not merely general and descriptive, but starts from the careful collection of data, proceeds to the critical discussion of them, and continues by a comparison of the results with those obtained in similar observations in other regions.

To take a single branch of Physical Geography, the study of Rivers, the amount of accurate material which has been adequately discussed is small. In our own country the rainfall of various river basins is well known through the efforts of a meteorological Association, but the proportion of it which is removed by evaporation, and of that which passes into the soil, has only been very partially studied. Passing to the run-off, which is more easy to determine satisfactorily, the carefully measured discharges of streams and rivers are not nearly so numerous as they should be if the hydrography of the rivers is to be adequately discussed; for although the more important rivers have been gauged by the authorities responsible for them in many cases, the results have usually been filed, and the information which has been published is usually a final value but without either the original data from which it has been deduced, or a full account given of the methods of measurement which have been employed. For the requirements of the authority concerned such a record is no doubt adequate, but the geographer requires the more detailed information if he is to co-ordinate satisfactorily the volume discharged with local rainfall, with changes in the rates of erosion or deposition, and the many other phenomena which make up the life-history of a river. Here too it is usually only the main stream which has been investigated; the tributaries still await a similar and even fuller study. A valuable contribution to work of this kind exists in the hydrographical study of the Medway and of the Exe which has been undertaken by a Committee of the Royal Geographical Society during recent years, and this may serve as a guide to other workers; but, however welcome such a piece of work may be, I should much prefer to see the hydrography of a tributary of a river system worked out by a geographer as a piece of individual work, just as the geology or the botany or the zoology of a single restricted area is investigated by those whose interests are centred in these subjects.

In the same way we still know too little of the amounts of the dissolved and suspended matter which is carried down by our streams at various seasons of the year and in the different parts of their course. This class of investigation does not need very elaborate equipment, and may provide the opportunity for much useful study, which may be extended as information is increasingly acquired. In this way when numerous individual workers have studied the conditions prevailing in their own areas, and traced them through their seasonal and yearly variations, we shall possess a mass of valuable data with which we may undertake a revision of the results which have been arrived at in past years by various workers from such data as were then at their disposal.

In this one branch of the subject there is ample scope for workers of all interests in the measurement of discharges, in the determination of level, and of the movement of flood waves, in determining the amount of matter transported both in suspension and in solution, in tracing out the changes of the river channel, in following out the variation of the water-table which feeds the stream, in ascertaining the loss of water by seepage in various parts of its course, and generally in studying the hundred other phenomena which are well worth investigating, and which give ample scope for workers of all kinds and of all opportunities. There is work not only in the field, but also in the laboratory and in the library which needs doing, for the full account of even a single stream can only be prepared when data of all classes have been collected and discussed.

On the Scottish lakes much valuable scientific work has been done, and also on some of the English lakes, so that excellent examples of how such work should be done are available as a guide to anyone who will devote his spare time for a year or two in making a thorough acquaintance with the characteristics and phenomena of any lake to which he has access.

Coast-lines provide another class of geographical control which repays detailed study, and presents numberless opportunities for systematic investiga-

tion and material for many profitable studies in geography. The shores of these islands include almost every variety of type, and furnish exceptional opportunities for research of a profitable character, especially as lying on the border-line between the domain of the oceanographer on the one hand and the physiographer on the other. The precise methods of representation which are possible on the land have to give way to a more generalised treatment over the sea, and the shore line is liable to be handed over to the latter sphere, so that there is much interesting and useful work open to anyone who will make an accurate and detailed study of a selected piece of coast-line, co-ordinating it with the phenomena of the land and sea respectively.

The teaching of Professor Davis in pressing for the employment of systematic methods in describing the landscapes with which the geographer has to deal has brought about a more rational treatment, in which due recognition is given to the structure of the area, and the processes which have moulded it, so that land forms are now for the most part described more or less adequately in terms which are familiar to all geographers and which convey definite associated ideas, in the light of which the particular description is adequately appreciated. It has been urged by some that such technical terms are unnecessary and serve to render the writings in which they occur intelligible only to the few; that anyone should be able to express his meaning in words and sentences which will convey his meaning to all. There is no great difficulty in doing this, but in such descriptions to convey all that a technically-worded account can give to those who understand its terms would be long and involved on account of the numerous related facts which would be included. It is consequently essential in all accurate work that certain terms should have very definite and restricted meanings, and such technical terms, when suitably chosen, are not only convenient in that they avoid circumlocution, but when used in the accepted sense at once suggest to the mind a whole series of related and dependent conditions which are always associated with it.

The compilation of a glossary of geographical terms has been in progress in this country for many years without having reached finality, and much of the difficulty which has been experienced is doubtless due to the fact that so many words have not been consistently used with a well-defined meaning. Such looseness of expression is more liable to occur in the case of foreign words which have been imported in the first case by writers who are not scientifically trained, and therefore do not use them in connection with a specified set of conditions. This, however, is unimportant if only scientific geographers, when they accept a term as a desirable addition to the geographical vocabulary, will associate it definitely with such conditions and use it consistently in that connection. As an instance I may quote the word "sadd," which etymologically means to block, or stop. This term was naturally and reasonably used to indicate masses of uprooted marsh vegetation which had been carried along by the current and, if checked at a sharp bend or a narrow point of the stream, blocked the channel. So long as it is used in this restricted sense it is a useful term to describe a phenomenon which occurs under certain definite conditions and which leads to equally well-defined geographical results. This use of it is associated with a meandering river channel in an alluvial flood plain, where shallow lagoons occur, in which such marsh vegetation grows luxuriantly; when this vegetation is uprooted by storms and carried by the rising water into the main stream it provides the drift material which makes up the block or 'sadd.'

But this term has been extended immoderately to mean the region in which these physical conditions occur, or the type of vegetation which grows under these conditions, and even the type of country where such conditions prevail. One writer has even used the word in describing fossil vegetation of a character such as is associated with marsh lands.

The crystallisation of such geographical terms into true technical terms is an important step in the furtherance of scientific geography, but it must be done by the geographers themselves, and no means of doing this is more fruitful than the work of original research and investigation in definite areas or on specific problems.

It would take too long to discuss each branch of physical geography and indicate the opportunities for individual effort, but what has been said of one

may be said of all the others. Not only in all parts of the Empire but in these islands also there is ample opportunity for the detailed geographical study of single localities or individual phenomena, just as much as in geology, in botany, or in zoology; and it is these separate pieces of work which, when thoroughly carried out and critically discussed, provide the material on which wider generalisations or larger investigations can be based. Herein lies, therefore, the importance of the prosecution of them by as many workers as possible, and the value of communicating the results to others for criticism and for comparison with the results which they have obtained; for such work, if it cannot be made accessible to other workers in the same and related fields, loses a large proportion of its value.

If we now consider some of the problems of human geography we shall find the need for such systematic study to be even greater; for the variable factors involved are more numerous than in physical geography, and many of them are difficult to reduce to precise statement; the quantitative study of the subject is therefore much more difficult than the qualitative or descriptive, so that the latter is too frequently adopted to the exclusion of the former. The remedy lies, I believe, in individual research into special cases and special areas where the factors involved are not too numerous, where some of them at least can be defined with some accuracy, and where, consequently, deductions can be drawn with some precision and with an accuracy which gives grounds for confidence in the result. The settlements of man, his occupations, his movements in their geographical relations are manifested everywhere, and subjects of study are to be found without difficulty, but their investigation must be based on actual observation, and on data which have been carefully collected and critically examined, so that the subject may be treated as completely as possible, and in such a way that the evidence is laid before the reader in order that he may form his own conclusions.

It is probable that some of the lack of precision which is to be found in this part of the subject is to be attributed to the want of precision in its terminology. For many things in human geography good technical terms are required, but these must be selected by those who have studied the type or phenomenon concerned and have a clear idea of the particular conditions which they desire to associate with the term; this is not the work of a Committee of Selection, but must grow out of the needs of the individual workers.

There is, it must be admitted, no small difficulty in using the same preciseness of method in this portion of the subject as is readily attainable in mathematical geography, and is usually practicable in physiography; but at any rate it is undesirable to indicate any condition as the controlling one until all other possible influences have been carefully examined and have been shown to have less weight than that one which has been selected.

Whether the investigation deals with the settlements of man or his movements and means of communication it is important that in the first instance problems of a manageable size should be undertaken and thoroughly treated, leaving larger areas and wider generalisations until a sufficient stock of thoroughly reliable material which is in the form in which it can properly be used for wider aims is available.

The relation of geographical conditions to small settlements can be satisfactorily worked out if sufficient trouble is taken and all possible sources of information, both of present date and of periods which have passed away, are utilised. Such studies are of a real value and pave the way to more elaborate studies, but we need more serious study of these simpler cases both to set our facts in order and to provide a methodical classification of the conditions which prevail in this part of the subject. Out of such studies there will grow such a series of terms with well-defined associations as will give a real precision to the subject which it seems at the present time to lack.

The same benefit is to be anticipated from detailed work in relation to man's communications and the interchange of commodities in all their varied relations. Generalised and descriptive accounts are readily to be found, and these are for the most part supported by tables of statistics, all of which have their value and present truths of great importance in geography, but the spirit of active research which aims at clearing up thoroughly a small portion of the wide field

of geographical activities has unequalled opportunities in the somewhat shadowy relations between the phenomena which we meet in this part of the subject, for focusing the facts better, and obtaining a more exact view of the questions involved.

Where the geography of States (political geography) is concerned the same need for original investigation as a basis for generalisations may be seen. At the present time there is much said about the various boundaries of States, and in general terms the advantages and disadvantages of different boundaries under varied conditions can be stated with fair approximation to accuracy. But I do not know of many detailed examinations of these boundaries or portions of them where full information of all the factors involved can be found set out in an orderly and authoritative manner, thus forming a sure foundation for the generalised description and providing the means of verifying its correctness or revising it where necessary.

Perhaps there is really more scientific research in geography being undertaken by individuals than I have given credit for, but certainly in geographical periodicals, and in the bibliographies which are published annually, the amount shown is not large; neither is the number of authors as large as might be expected from the importance and interest of the subject and from the activity of those centres where geography is seriously taught. There seems to be no reason why individual research on true scientific lines should not be as active in geography as it is in geology, botany, zoology, or any other branch of knowledge; and, just as in these, the real advance in the subject is dependent on such investigations rather than on travels and explorations in little-known lands, unless these too are carried out scientifically and by thoroughly trained observers who know the problems which there await solution, and can read the evidence which lies before them on their route.

If research in these directions is being actively prosecuted, but the appearance of its results is delayed, let us seek out the retarding causes if there be any, and increase any facilities that may be desirable to assist individual efforts.

Short technical papers of a thoroughly scientific character, such as are the outcome of serious individual research, are, of course, not suitable for those meetings of Geographical Societies where the majority of the Fellows present are not scientific geographers, but should be presented to small meetings of other workers in the same or allied fields, where they can be completely criticised. The reading, discussion, and the publication of papers of this class are for geography a great desideratum, for it is in them and by them that all real advance in the subject is made, rather than by tales of travel, however interesting, if these are not the work of one trained in the subject, having a knowledge of what he should observe, and of what his predecessors have done in the same field. The regional aspect of geography in the hands of its best exponents has given to young geographers a wide and comprehensive outlook on the interaction of the various geographical factors in a region, the responses between the earth's surface and the life upon it, and the control that one factor may exercise upon another. In this form the fascination of geographical study is apparent to everyone, but I sometimes wonder whether the exposition of such a regional study by one who is thoroughly master of the component factors, having a first-hand knowledge of all the material involved, and knowing exactly the reliability of each portion, impresses sufficiently upon the student the necessity of personal research into the details of some problem or phenomenon in such a way as to gain a real working acquaintance with them; or does it on the other hand tend to encourage generalisations based on descriptive accounts which have not been verified, and where coincidences and similarities may be accepted without further inquiry as evidence of a causal connection which may not really exist? I imagine that the student may be attracted by the apparent simplicity of a masterly account of the geographical controls and responses involved, and may fail to realise that geographical descriptions, even though technically phrased, are not the equivalent of original quantitative investigation, either for his own education or as a contribution to the subject.

For these reasons I believe that Societies can do far more good in the promotion of geography as a science by assisting competent investigators, by the loan of books and instruments, and by giving facilities for the discussion

and publication of technical papers, than by undertaking the investigation of problems themselves.

Among the earlier Presidential Addresses of this Section some have laid stress on the importance of the recognition by the State of geography in education; others have represented the great part which the Geographical Societies have played in supporting and advancing the subject; others again have urged the fuller recognition of geography by Educational Institutions. I would on this occasion attach especial importance to the prosecution of serious research by individuals in any branch of the subject that is accessible to them, to the discussion of the results of such work by others of like interests, and to the publication of such studies as having a real value in promoting the advancement of scientific geography.

British Association for the Advancement of Science.

SECTION F: MANCHESTER, 1915.

ADDRESS

TO THE

ECONOMIC SCIENCE AND STATISTICS SECTION

BY

PROFESSOR W. R. SCOTT, M.A., D.PHIL., LITT.D., F.B.A.,

PRESIDENT OF THE SECTION.

THE economists of great distinction who have presided over this Section of the Association in past years have usually addressed themselves to the discussion of the progress of Economic Science in relation to some problem which had become striking or significant at the time when each meeting was held. It has fallen to my lot to prepare an address at a period when the Empire is involved in a war of tremendous moment both to our country and to the world. Not the least dominant phase of this epoch-making struggle is the economic one; and it is inevitable that, on this occasion, consideration should be given to some of the reactions of this great war upon industry, credit, and finance.

It is both remarkable and significant how silent British economic theory has been upon what may be described as 'the economics of war.' No doubt there are volumes, treatises, and isolated passages which record the effect of some specific war upon prices, or upon credit, or upon the national finances. Or, again, other works may deal with some practical inconvenience which the writer experienced; but, when the total result is estimated, it will be found that by far the larger part of the scanty discussions of this subject are either purely historical or else purely practical. In the vast majority of cases our writers have confined themselves to an analysis of the effects of some specific war on finance and commerce with a view to suggesting measures towards counteracting the inevitable losses, instead of studying the principles of war in general with a view to strengthening the national resources in preparation for future hostilities. Thus, while British economists have said something about former wars, they are almost wholly silent concerning wars to come. This is a fact of immense significance. It demonstrates beyond the possibility of doubt or cavil that in this country there has been no such thing as a mobilisation of economic opinion. On the contrary, our economists can claim with justice that they have been ever on the side of the world's peacemakers, not with false lip-service but through serious and sustained reasoning.

Once Mercantilism began to decline, it is astonishing how little one finds in British economic literature relating to causal relations between war and industry. What there is usually appears as a side issue in some other investigation. For instance, at the end of the seventeenth century, during the eighteenth century, and in the early years of the nineteenth, there was a long controversy over the nature of credit, with frequent digressions upon the character of public debts, which was in effect the consideration of the financing of past wars. In its extremest form one theory represented public borrowings as 'a mine of gold'—a statement which influenced both theory and practice during the eighteenth century. The exaggeration of 'the fund of credit' no doubt seems strange and almost laughable to us now, but it does not differ greatly in principle from the vague popular opinion that a nation can become

richer by increasing its taxes. A public debt as the Midas of the eighteenth century is as much a fairy tale as the modern conception of taxation as a species of 'manna falling on the country in a fertilising shower.' Naturally there was a reaction from the magic claimed for a state-debt, and the opposed type of thought urged that supplies, even for war, should be raised during the period in which the expense is incurred. The citation by John Stuart Mill of a passage from Chalmers, in which the latter view is expressed, is almost the only echo of this controversy in more recent times. During the last fifty years, if a few occasional writings, such as those of the late Sir R. Giffen 'On Consols in a Great War,' [1] be excepted, our standard economic works have scarcely anything to say on war, and there is nothing which can be construed into a preparation for hostilities.

But the cultivation of peace by British economists in avoiding the study of the mobilisation of national resources for war has not merely been negative; it was also positive in proving the advantages of peace and the tendency of enlightened economic views to promote it. More than two hundred years ago Sir Dudley North wrote that 'the whole world as to trade is but as one nation or people, and therein nations are as persons. The loss of trade with one nation is not that only, separately considered, but so much of the trade of the world rescinded and lost, for all is combined together.' [2] In the same spirit David Hume urged that 'our domestic industry cannot be hurt by the greatest prosperity of our neighbours.' [3] Before the end of the eighteenth century men of open mind not only recognised that war was a great evil, but also that there was nothing in international commercial relations to cause it or justify it. And so Burke spoke of the condemnation of war as a commonplace and 'the easiest of all topics.' Even victory accompanied by substantial material gains is described by Hamilton as but 'a temporary and illusive benefit.' In one passage he writes: 'The emphatic epithet of "the Scourge of God" has been aptly bestowed upon the extensive warrior. . . . Riches, thus collected, no more resemble riches acquired by industry in advancing the happiness of the nation than the mirth of intoxication is worthy of being compared to the permanent flow of spirits which health and activity confer.' [4] The undercurrent of the work of all the great British economists has been ever on the side of peace. Adam Smith suggested measures to prevent wars being undertaken wantonly.[5] Ricardo shows how free commerce 'diffuses general benefit and binds together by one common tie of interest and intercourse the universal society of nations throughout the civilised world.' [6] It would be wearisome to multiply quotations from the long line of great writers, for already enough has been said to prove that the encouragement of the best possible relations with other countries has always been a prominent feature of their teaching.

This conclusion leads on to the discussion of a new problem. May it not be urged that British economists have been either too selfish or too idealistic—too selfish in inculcating material welfare as an end to the neglect of those national interests which are now seen to be vital, or too idealistic in seeking a cosmopolitan golden age which has proved to be but a dream? That is in fact, have not our economists in their devotion to peace neglected the economic preparation for war? While it is true that the essential teaching of the master minds has been thoroughly pacific, at the same time they recognised that, while war was an evil, both to the world and to us, it was one that might be forced upon the nation. But it would be a dangerous error to conclude from the rare mention of warfare in our economic literature that economists had no ideas upon the subject. Adam Smith has shown with considerable detail that the

[1] *Works*, ii. pp. 189-203. The calculation was that Consols would fall 15 per cent. at the opening of hostilities. The fixing of a minimum price during the first months of the war has made it impossible to confirm or refute Giffen's forecast.

[2] *Discourses upon Trade* (1691), p. viii.

[3] *Essays*, i. p. 347.

[4] *Progress of Society* (1830), p. 411.

[5] *Wealth of Nations* (ed. Cannan), ii. p. 411.

[6] *Works*, pp. 76, 160.

sinews of war consist of consumable goods.'[7] Therefore, since his time it was recognised that, if war should come, the strength of the nation on the economic side was to be found in the efficiency of its productive system, in the soundness of its credit and finance, and in the success of its schemes of social betterment which provided a vigorous and patriotic population. To have contributed something towards the making of free men in a free land is an achievement of which the economists of this country have no reason to be ashamed. Moreover, with freedom there is the power of initiative and organising ability. And if more than twelve months of war have taught us anything, it is how much modern warfare involves just those qualities of initiative and organising ability which are required for the successful prosecution of industry and commerce. To the economist it must be a matter of profound regret that circumstances have made it necessary to divert these powers from the arts which sustain and brighten life towards causing the evils of death and destruction. Still it is the hard and grievous fact with which we have to reckon; and, to make the reckoning complete, account has to be taken of the genius of our people in which the work of British economists may claim to have some share. We should not be misled by that curious national trait which no foreigner ever completely understands—namely, our inveterate habit of praising the methods of our rivals as if they were unapproachable in their excellence. In the seventeenth century it was the Dutch who were said to be our commercial masters, and very similar things were written later about the French. Therefore, to everyone who is patient enough to look beneath the surface, there is no reason to be perturbed by the commonplaces that are to be found in every newspaper concerning 'the triumphs of German organisation.' No doubt there is very much we can learn from them in systematic arrangement, but what is of first-rate importance is the different spirit that informs the two methods. German organisation involves a mechanical rigidity, and its initiative is severely limited. Ours, on the other hand, is spontaneous and free. No doubt it is slower in starting—often it may seem to us to be painfully slow—but what it can achieve in the end is something greater, for it is the expression of the free soul of a free people. Therefore, for this reason alone, there can be no doubt as to the successful result, for, whether the time required be long or short, the goal of victory must be reached by that nation which can bring initiative to bear upon the economic side of war. And, however much we may have suffered at the beginning from the peaceful habit of mind that limited our preparations to a bare minimum, we have in our industrial organisation, however much at times we may depreciate it ourselves, a wonderfully developed instrument, which only needs to be made available for supplying the almost innumerable needs of modern armies. That there has been delay in making some parts of it available as quickly as was desirable and seemed possible, arose in part from the conditions under which our system has grown up and under which it works. Freedom of enterprise depends to a very large extent on the circulation of rapid and reliable information. British initiative has been accustomed to base its judgments upon data collected from various sources. Modern warfare has introduced secrecy and the suppression of news. This, it appears to me, has been one cause, and perhaps the main one, for the slowness of the adjustment of our organisation to war conditions. Initiative has been deprived of one of the important aids upon which it was accustomed to rely. Therefore the problem, which it is to be hoped is at present in process of solution, is how to avoid the disclosure of information which might be of value to an enemy and at the same time to supply our productive workers with sufficient data to enable them to form accurate opinions as to how their efforts can best help the national cause.

In a country in which the ideal of peace has flourished there must always be a considerable dislocation of industry when it diverts its peace-organisation to the purposes of war. As regards Great Britain that dislocation has exerted its force in two distinct waves. First there was the mobilisation and then the recruiting for the new army, concurrently with which there was the diversion of demand caused by the provision of the manifold needs of the forces. At the beginning of the present year this first change might be described as

[7] *Wealth of Nations*, i. p. 407.

having approached completion, though necessarily the maintenance of reinforcements involved a steady drain on the number of workers. But in the early summer the campaign for increase of munitions brought about a further dislocation. This was a minor one in point of numbers involved, but it has to be noted that it was likely to produce a disproportionate effect upon industry owing to the normal floating supply of labour having already been used up. When the latter change is completed it is to be hoped that, apart from minor adjustments, the transition will be accomplished and the national industry will be established on a war-basis. The two most critical periods occasioned by war are first the change from peace organisation to war organisation, and secondly the converse change after the conclusion of hostilities on a large scale. Ricardo pointed out long ago that the outbreak of war after a long peace was likely to cause distress and a commercial crisis. The great expansion of credit since the last great war introduced an added difficulty. The improvement of transport and communication has linked the whole world together by tenuous filaments of credit. These had proved sufficient to bear a normal strain, but one must experience a certain amount of apprehension when these delicate threads were rudely hacked and hewn by the sword. The financial interests of the country, like the class of *entrepreneurs*, were confronted suddenly with totally new conditions. The old landmarks were gone, and at first a certain amount of blind groping was inevitable. The leaders in finance and industry were suddenly involved in the fog of war, and the compass by which they were wont to steer proved unreliable. Moreover, the situation was such that quick decisions were called for, just when rapidity of correct judgment was peculiarly difficult. The most urgent problem was the maintaining of the credit of the banks amongst their depositors. Here the essential soundness of the credit-system in July of last year was of paramount importance. Credit resembles a highly elastic body: if it is greatly expanded a comparatively slight pressure may cause a rupture; if, on the other hand, it is not unduly distended, it will bear a shock, though with some quaking, which would shatter a more solid substance into fragments. The comparative equanimity of depositors, added to the inherent soundness of the banking system, was a feature of great strength in times which were in the highest degree anxious. The closing of the Stock Exchange and the temporary breakdown of the foreign exchanges made some measure of external assistance from the State essential, though in the future there will no doubt be considerable discussion amongst economists as to the precise form which it should have assumed.

An unexpected outbreak of hostilities is experienced first in the domain of credit, but the disorganisation soon manifests itself throughout the whole range of productive processes. In the general upheaval the normal course of demand is shifted to an unusual extent. That for all kinds of supplies for the forces at once increases, while the consumption of other kinds of goods is subject to considerable fluctuations. Some raw materials are no longer obtainable, having been wholly produced in countries with which communication has ceased, others are procurable only in reduced quantities, while the supply of others was at first uncertain. Again, the state of credit reacted on foreign trade, rendering exporting difficult and in some cases impossible for a time. All this means that a large diversion of labour and capital became necessary in the first months of the war; and again in the spring of this year the insistent demand for more and more munitions added to the dislocation. With the progress of specialisation in industry there was the apparent risk that such diversion of productive power could only be accomplished at great sacrifice. It would seem that the greater and greater use of specialised machinery with the corresponding specialisation of skill would make the change very difficult, and one which would involve great loss of capital and unemployment. After a year of war we see that the latter problem has dropped below the horizon, though it is likely to emerge again on the return of peace when the converse change from war conditions to peace conditions takes place. As regards capital, manufacturers have developed the adaptation of men and machines to certain special purposes. In many cases the demand for the products of these industries has diminished very greatly, and it would seem that the fixed capital must remain either partly or wholly unemployed during the war. Recent economic investigation has shown

that industry not only proceeds by separating processes of production, but also in surmounting the lines of division formerly regarded as distinct. Thus Dr. Marshall has shown that the operatives in a watch-making factory could work the machines used in gun-making or in sewing-machine-making, or in the making of textile machinery.[8] The experience of the early months of the war has fully confirmed the anticipations of economic theory as to the power of transference of specialised capital and labour from one process (for which the demand has temporarily declined) to another in which it has increased. It is not remarkable that cotton operatives should migrate to woollen mills to make khaki, but it might at first occasion surprise to hear that many makers of brass door-handles soon were at work in helping to produce shrapnel shells—their contribution consisting of the brass driving-rings and copper bands. At the beginning of the winter machines that formerly made spokes for cycle wheels produced knitting needles. Plant normally used to make gear-cases turned out hollow ware tins and basins for the troops. Pen-making factories found new employment in manufacturing military buttons. The list of war uses for plant during the first months of hostilities could be very greatly extended, and the establishment of the Ministry of Munitions has added immensely to the employment of plant for war purposes; but enough has been said to show that economic theory has been proved right in anticipating a large measure of recuperative power in productive processes enabling them to re-employ under the new conditions capital and labour which were temporarily idle. All this is satisfactory for the war period; it must be remembered that on the return of peace the reverse change will have to be made. There may be a short trade boom (arising out of the attempt to restore some of the material ravages of war), but the joint demand from it and from the trades re-opened is likely to be considerably less than the huge present expenditure on manufactures for war. Thus the unemployment occasioned by dislocation of industry through hostilities is likely to be carried forward as a species of suspense account which must be liquidated not very long after peace. Moreover, international credit is likely to re-act on the situation in a prejudicial manner. Even already the financial system of Germany is more strained than appears on the surface. This fact is advantageous to us as belligerents, but it will probably be prejudicial to us not long after the re-establishment of peace. At present much of the inconvertible paper circulating on the Continent does not affect us here. When the inflation has to be squeezed out after the war, a disturbance of credit is not unlikely.

Important as the flexibility of capital and labour have been, the striking success of maintaining our communications within the Empire and with neutrals has been even more remarkable. Steam and wireless telegraphy have had the effect, when supported by adequate naval strength and preparation, of simplifying the protection of maritime trade routes. The events of the early months of the war afford a brilliant justification of the views of many economists of the advantages of diversified sources of supply of food and raw materials from the colonies and foreign countries. The later operations of German submarines against our commerce and even against passenger ships can bring no real advantage to the enemy, and one cannot find words to describe adequately the infamy of the sinking of the *Lusitania*. Some of the destruction of trawlers and drifters cannot pay the cost of torpedoes, that of the rest is at the worst an inconvenience, but in material loss it is incomparably less than the damage of property which is happening every day on the Western battle front when villages and towns are destroyed by artillery fire.

The inestimable services of the Navy in the general protection of sea-borne commerce may be illustrated to a partial extent by reference to the last occasion on which our maritime trade was subject to serious interruption, namely, during the years of hostilities between 1793 and 1815. At that period Great Britain possessed an overwhelming naval superiority, yet freights and marine insurance were often extraordinarily high. For instance, these charges on hemp and tallow from Petrograd to London were ten times the normal rate. Insurance on hemp was 20 per cent. to 40 per cent. of the value. In some cases the freight

[8] *Principles*, p. 339.

and insurance of flax were more than the prime cost. These were moderate rates for that war period. Take the case of silk. It cost 100*l*. to bring a bale of 240 lb. from Italy, instead of the previous rate of 6*l*. These figures seem almost incredible, but they are vouched for by Tooke.[9] Further, they were only a part of the increased difficulty in transport. The delay was remarkable. It is recorded that on one occasion it took a year, on another two years, to send a parcel of silk from Italy to England. Interest on capital and disarrangement of manufacture during the extra period of transit might be estimated to add another 30*l*. to the cost of conveying a bale of silk—that is, 130*l*. against 6*l*.; so that altogether the cost of transport and allied charges increased by more than twenty times the amount paid in times of peace. Such, in bald numerical terms, is the debt we owe to the silent watch and ward of the Navy, which is of equal benefit to our Allies also.

So far I have discussed questions which relate mainly to organisation and transport; but, in summing up our economic position in the present war, the provision of resources by the various combatants will become increasingly important. When Germany cast the sword of Brennus into the scales of international justice she must surely have forgotten the ultimate influence of the wealth and resources of the British Empire. 'To face the world in arms in shining armour' may seem heroic to the Teutonic mind, but it is futile provided that the resources of the world are rightly used against her. This it appears to me is at once our opportunity and our responsibility. War has become so complex that to conduct it upon a great scale demands large capital resources. Our past savings, supplemented by those made during the war, constitute the reserve of the credit of the Allies. No doubt, as in the case of organisation, time will be required to make the full extent of the pressure felt, but it is pressing slowly but inexorably upon the enemy, and as the struggle develops it will press with increasing power. Given the necessary fighting strength of good quality, its efficiency depends upon the extent and adequacy of its supplies. If the struggle be protracted, then victory will rest with the side which can best maintain its supplies, and it is here that our wealth is likely to be a decisive factor. But it must be brought to bear in the right way, and in this respect important functions devolve upon the non-combatant. For many years public and private economy has been a forgotten virtue—too often it came near to being regarded as akin to a vice. Now our ostensible leaders of public opinion are preaching economy almost as if they had discovered a new religion. Such missionary zeal, even though belated, is advantageous. War makes great changes in Distribution; and changes in Distribution, when the general standard of living has been rising rapidly, are likely to lead to extravagance, more especially in war-time when all conditions favour waste. But economy, necessary as it is, can be no more than a step. What is required is the maximum supply of goods, in excess of the needs of the civilian population, which will maintain and even increase the efficiency of the fighting forces. In the summer attention was concentrated on munitions, and this is an instance of our national habit of concentrating on the more pressing aspect of some highly complex problem. The effectiveness of the gunner on a war-ship or of the soldier in the firing line requires the product of the labours of many workers: without the full supply his value as a fighting unit deteriorates. Therefore it devolves upon us to supply such goods both for our own forces, and to a certain extent for some of our Allies also. The effect of public and private economy is to leave more wealth in the hands of the taxpayers, but much of that wealth does not consist of commodities which avail for augmenting the power of the forces. To effect the necessary transformation such wealth must be transferred from the owner of it, either in the form of taxation to the State or in a subscription to a public loan. The Government then arranges for the acquisition of the commodities it requires either by making them here or purchasing them, whether in this country or abroad. In some cases it may be more advantageous to acquire the goods we need from foreign countries by exchanging our own products for them. Now, we already import considerable quantities of food and other necessaries, and therefore our purchases outside this country for war purposes constitute an addition to these imports. Against this we have

[9] *History of Prices*, i. p. 309; *Thoughts and Details of High and Low Prices*, pp. 129, 211.

the profits of our shipping and the income on capital invested abroad and in the colonies. The aggregate of the former is likely to be reduced through the war, and there may be a temporary reduction in the latter through the same cause. Also there are our visible exports and some minor items. Thus it follows that the situation demands as large as possible a production of goods consisting first of supplies for the forces produced at home, secondly the home supply of the necessaries and simpler comforts of life, and thirdly goods to export to pay for our imports of military supplies and of food from the colonies and abroad. And this leads to an important conclusion—namely, that, after the maximum demands both of the naval and military forces for men have been met, there is a plain duty before those that are left. The exigencies of the times demand that there should be no idle class, whether of idle rich or idle poor. We have called out some of our reserves of fighting men, and we must draw also upon our reserves of workers. In the expressive language of our brothers from the Dominions overseas, 'it is up to the non-combatant at home not to let the fighting forces down,' but by his steady and sustained industry to help in providing, directly or indirectly, all the supplies which are required, either in helping to produce these or in making those goods which are exchanged for them. Thus there is a definite duty for every one of us, according to our varied capacities, to take part in a great national endeavour. This is plain common-sense. From the specially economic point of view, war is waste and loss. Therefore it is obvious that we cannot work too earnestly or too unsparingly to bring about as soon as possible the cessation of that loss and a return to normal conditions. No doubt, here again organisation is required. The people are not in a position to judge as to the balancing of the needs for reinforcement, for labour for military supplies produced in this country and for labour to produce goods to be exchanged for supplies or food imported. All the more it becomes necessary for the authorities to strike a balance and to issue clear and unmistakable directions.

All this must seem far removed from the principle of *laissez faire*, the operation of which has become more and more restricted by the mass of governmental regulations and emergency measures. But the people assent to the restriction of their liberty of action under an imperious necessity. Because sacrifices are made in a national emergency, without complaint or murmuring, it by no means follows that the public is learning to love its chains. Unless the war makes a radical change in the national temperament, it would be a political mistake of the greatest magnitude to retain restrictions upon commerce even a week longer than these are unavoidable. In the confused issues of warfare we have the unshakable conviction that we are staking the lives of our soldiers and the whole resources of the British Empire in defence of liberty. It would be a tragedy if, in the defence of liberty, freedom of enterprise and labour were sacrificed, for victory in war would be tantamount to the defeat of our national ideals.

In all the long history of this Association, it has never before fallen to the one who presided in this Section to survey such a scene of ruin and devastation. To the economist war must ever be the pre-eminent instance of wicked waste. One is almost tempted to discuss again that old problem, debated by Bishop Butler—namely, whether whole nations may become temporarily mad. Yet out of all the suffering and all the loss, something that is necessary to the progress of the world must emerge—something that, as things are, can only be won by sacrifice and sorrow. It has happened before in the history of civilisation, and it has now unfortunately occurred again, that it is needful to defend existing institutions from attacks which menace not only these but the possibility of future development. The sanctity of a nation's plighted word must be maintained as a basis for the stability of international relations. One issue which is involved in the present war is the whole basis of international contract. Without being unduly optimistic one may hope that some compensation for the vast destruction it has caused may be found first in the establishing of treaty rights on a secure foundation, and then that a way will be opened for international agreements which will lessen the risk of future wars. Moreover, the inviolability of public faith is not only of supreme importance in the political sphere, it lies at the root of the whole mechanism of foreign trade and the international money-market. The new 'scrap of paper' theory constitutes a bankruptcy of

external credit. It recoils with crushing force on the nation whose good faith has become suspect, and it produces a feeling of doubt and insecurity throughout the money-markets of the world. When one remembers Belgium, it is not a little remarkable that one of the best analyses of the causes which determine foreign estimation of a nation's credit has been written by a German. I quote the concluding summary: 'These causes are to be found in the opinion which the world holds of a nation's political standards, of the soundness of her institutions, the inviolability of her pledged word, in the last resort of the moral principles which inspire and the intellectual faculties which direct her people's activities.' [10]

Further from the economic standpoint this war is one which, provided it ends decisively in favour of ourselves and our Allies, should free us from a menace which has faced this country for a generation. At each great epoch in our history, it has been our duty to prevent the wreck of civilisation through the appearance of a new Iron Age with its doctrine that wealth is the prey of the stronger. And so England resisted Spain, Great Britain Napoleon, and now the British Empire confronts Germany in defence of the principle that force must not triumph over law. Indeed, the present strife is perhaps the only issue from a situation in Europe that was becoming intolerable. Year after year the nations on the Continent were proving their devotion to peace by arming to excess, as they said, to defend peace. The burden grew heavier and heavier, diverting national resources from the improvement of the condition of the people and the growth of commerce. Before the war the annual expenditure of the Powers of Europe on their armies alone had increased to about 290,000,000*l.* There can be little doubt that much of this outlay, as well as that on navies, could be saved. It is to be hoped that, when a durable peace has been signed, a very large saving in this type of expenditure will be effected. Moreover, an abatement of military preparations should have another effect in diminishing the drain on productive processes through compulsory military service. Thus, on the whole, while the losses of the war will be enormous, there are some gains, largely of an immaterial kind, to be placed on the other side of the account—namely, security and the re-establishing of international contract, and, of a material kind, in a possible diminution of the burden of armaments, both direct and indirect.

A special aspect of the problems under discussion is the provision of capital for the re-starting of trades contracted by the war and for the restoration of Belgium and other regions desolated during the progress of hostilities. Chalmers, writing a hundred years ago, supposed that in cases of this kind 'in a very few years the recovery both of population and labour would be completed.' [11] The explanation he gave was far from satisfactory even for the time at which it was written, and it is still more deficient as applied to the present circumstances, when in industrial countries fixed capital is much more important than in Chalmers' day. In the last quarter of a century any great catastrophe, such for instance as the partial destruction of San Francisco by earthquake and fire, has been repaired with comparative ease by bringing capital from outside. But the waste of war renders capital exceedingly scarce; in fact, a famine of capital after the war has been predicted. Such an anticipation is over-pessimistic, but capital is likely to be obtainable for a time only with some difficulty. It is to be feared that after the war Europe will experience very considerable straits for several years to come. Not only must the waste of war be made good, but its evil legacy in inflated funded and floating debts must be gradually dealt with, lessening by reason of increased taxation the normal margin for new savings. Increased work and greater economy are the only remedies, aided by improved methods of production.

It is to be hoped that some of the inevitable loss will be repaired in time by better methods of organisation and by an accelerated rate of invention. The waging of a just war results in a quickening of the national spirit. It forces a nation out of the easy and well-worn paths of custom and convention. Thus, out of all the suffering and all the loss, some good will come. The large proportion of our young manhood, which has gone to serve the country on the

[10] *On Some Unsettled Questions of Public Credit*, by Prof. G. Cohn, in *Econ. Journal*, xxi. p. 217. [11] *Works*, xix. p. 141.

seas or in the field and which returns having looked death in the face without being afraid, will not take up life where it was left. The noble qualities that have been evoked by the stress of battle will remain and will influence civil life during the next generation. The outlook will be both broader and also more simple. Methods of social legislation and administration will become more direct and less timorous. The men who have dared greatly and who have endured will chafe against the rules that have been formed during easier times. Great wars tear away the veils which hide the essential needs of living, and reveal what is fundamental. The directness of vision that has faced danger is not likely to be alarmed in facing the difficulties of our social and industrial problems. And so we may expect with confidence that our legislation will be bolder and also more sane than it has been in the past. The sacrifices of so many cannot pass, when the war is over, and leave no trace. The nation has been re-vitalised in the course of the struggle and the influence of this movement will persist.

In many respects the economic problems that will confront us after the war will be even more serious, and certainly not less difficult, than those of the present time. Still there can be no doubt that these will be faced with courage and patience. The period of stress through which we are passing has shown the unity of thought and purpose throughout the whole Empire. And this, in spite of many appearances to the contrary, will be a great asset in the future. The great national emergency has caused a closing of the nation's ranks, and it rests with us to keep them firm and steadfast when peace returns. There are plain signs that it may not always be easy, since so many industrial and other difficulties have been carried forward as a suspense account which is to be dealt with when the war is over. National unity is enabling us to progress towards victory, and the same unity will be required to enable us to reap the full fruits of that victory at home. It would be a mad waste not to employ the qualities of heart and mind which have been aroused in this great struggle in the service of peace and social progress. The future may be difficult for some years to come, but difficulties are the opportunities of the strong and courageous. It has fallen to us to live in an heroic age; and, if we remain true to ourselves and to our high destiny, we shall have the strength and the fixity of purpose to achieve greatly in peace as well as in war.

British Association for the Advancement of Science.

SECTION G: MANCHESTER, 1915.

ADDRESS

TO THE

ENGINEERING SECTION

BY

H. S. HELE-SHAW, D.Sc., LL.D., F.R.S., M.Inst.C.E.,

PRESIDENT OF THE SECTION.

The preparation and delivery of a Presidential Address is usually a pleasant and not difficult task, although it seems to be the custom mildly to intimate to the contrary. In ordinary times the President chooses a subject on which he has done some work, and with which he is therefore familiar, and with which, moreover, his name is more or less associated. If this had been an ordinary time, I should have liked to deal with the fascinating subject of mechanical locomotion, and to review what has taken place, let us say, since the meeting of the British Association held in Manchester rather more than half-a-century ago. The subject would have afforded ample scope, as we can realise by considering what would have been the effect produced if the distinguished Engineer, Sir William Fairbairn, who was President of the British Association in that year, had told his audience that within a comparatively short space of time our roads would be to a large extent occupied with self-propelled traffic; that electricity, then nothing but a toy, would play a most important part in our means of locomotion, not merely for driving but for lighting. That it would be used for searching out and communicating with vessels far away from the land and from each other; that ships many times the size of the largest ones then in ordinary use would employ steam as Hero employed it two thousand years ago, and obtain by this means a speed more than twice that of any existing ships; and that many ocean ships would be propelled against wind and tide by engines without using any steam at all. If the President had further proceeded to predict that ships would travel under water for long distances as easily as on the surface, and that, above all, a safe pathway would be found in the air by means of machines flying at speeds far exceeding that of the swiftest birds, I suspect he would have lost a good deal of his reputation as a man of judgment and common sense.

Such addresses, which deal with scientific progress, are most instructive, and the historical treatment of engineering questions is of the utmost value in assisting the judgment as to future possibilities. It is therefore most disappointing that I do not feel justified in taking up your time with an address of this kind.

The fact is, the time is not an ordinary one, for the war which a year ago cast its shadow over the Meeting of the British Association in Australia has, as the months have passed by, gradually unfolded the most terrible page in the history of the world.

It is terrible not merely because of the frightful slaughter which has taken place and which will yet take place, owing on one hand to the gigantic armies employed, and on the other to the nature of modern warfare. A predecessor in the Chair, one who has left many marks of his genius on the peaceful engineering works of the country, Mr. Hawksley, commented about fifty years ago on 'the unhappy necessity of devoting so much of the science and skill of members of the Association to the defence of the homes of the people of this great

nation.' He further remarked with great prophetic insight: 'War is no longer carried on by means of mere animal courage and brute force; on the contrary, we perceive, much to our amazement, I believe, that the highest branches of mechanical science and the most refined processes of the mechanical arts are resorted to by the modern warrior for the purposes of offence and defence. And we are taught by the logic of the facts that the modern soldier must cease to remain a passive machine, but on the contrary, must henceforth be trained as a skilled labourer, if not indeed as a skilled artisan.'

I do not think, however, that either Mr. Hawksley or anyone else could anticipate what refinements of skill and science would be brought to bear, not merely on the destruction of the human species but of those ancient edifices of beauty which cost hundreds of years to build, and which cannot be replaced, or even the loss of those great works of engineering in the form of bridges and other structures which, though doubtless replaceable, represent the accumulated wealth of mankind. All this does not constitute, however, the worst feature of the war which is still raging. I do not know which will hereafter stand out in the blackest light—the callous disregard by our enemy of the recognised laws which have governed warfare amongst civilised countries hitherto, of which the recent murder of a defenceless submarine crew in neutral waters is an example, or the fact, of which there is now abundant and overwhelming proof, that this country, while animated only by peaceful intentions, was itself the real object and ultimate aim for the destructive effort of our enemy.

Fortunately we now all know that our determination at any cost to ourselves to stand by our treaty obligation to a weaker country was really a fateful moment in the history of our Empire. Had we then failed, such failure would have spelt our own doom.

This is not the place to dwell at any length on this subject; but I cannot help pointing out that the whole attitude of scientific and professional men in this country at the beginning of the war shows how little they realised the real nature of what was before us. Thus my own predecessor, after the war had begun, in his Presidential Address in Australia, used the words 'The discoveries in pure science, and their innumerable applications to practical ends, are ever a potent factor working for the common good.' The truth is that the great majority of us did not realise to what uses science would be put in the mutilation and destruction of our fellow creatures.

Still we are told by soldiers that practically any applications of science constitute legitimate warfare, and that the only way to escape from destruction ourselves is to employ all the resources of science in our own defence.

It is on these grounds that the Meeting of the British Association for the Advancement of Science has been held this year, because science is proving such an all-important factor in the present war.

The mere holding of this meeting, however, with a vague sort of idea that science is associated with war, does not seem to most of us to meet the real needs of the case. The decision to hold the meeting was made in March, i.e., six months ago. Since that time the nation has awakened to the fact that matters have become very much more serious, and we scarcely needed the solemn warnings of our responsible statesmen to enable us to realise this. We see our foe turning every resource towards the active prosecution of the war, and bringing in the aid of every man towards that end. If the result were a small matter, we might pursue our way, as we did at first, with the fatuous cry, 'Business as usual'; but day by day it is brought home to us that the Hymn of Hate, childish though it may seem, really represents the serious mind and deadly intention of our enemy. When I say this I know there are exceptions, and it is gratifying to find that at least one German guest of the British Association acknowledged in print the generous manner in which the German guests were treated in Australia after the war broke out, even being given a passage home.[1] This and much else warns us that our failure in this war means the loss of what has been built up in so many centuries, and what we value above all other things, viz., our freedom, and that this loss would be accompanied by atrocities and degradation beyond the most savage happenings of the

[1] I regret to say this forms a striking contrast to the brutal treatment meted out in many cases to British visitors in Germany.

past. It behoves us all then who are members of it to show that the British Association, which has rendered such great services to the country in the past, can bear its share of the burden to-day.

I assume that we are all agreed upon this point, and it remains to consider the best way in which such a work can be carried out. Understanding from the President that he is dealing with the question from the point of view of the whole Association, I need only deal with the matter as far as it concerns our Section. After discussing the matter with our Secretary and several of my predecessors in the Chair, I suggest that we continue the three Research Committees already in existence, but do not institute a fresh one, forming instead a Special Committee, the purpose of which I propose to lay before you. Before doing this, I should like to point out that the very fact that engineering constitutes such an important feature of the War has prevented our having, as often in times past, papers on military and naval subjects, such as Warships, Armour, Projectiles, &c. And this for two reasons: first, because nearly every professional or manufacturing engineer having as a rule sent the best of his staff to the fighting line is overwhelmed by actual work either directly or indirectly connected with munitions and war material, and is much better so employed than in talking about the subject, or even in attending the present meeting; secondly, because men who really know all about such work would not be likely to discuss it in public at the present time,[2] and I am sure you will agree with me that we do not want merely popular science on war subjects.

Hence, the twenty-four papers before the Section deal with engineering science generally, but I venture to think they are of a high scientific quality, and quite as important in character as those of former years; and it will be noticed in several cases they touch closely on subjects bearing on the War, such as those on wireless telegraphy and traction.

The object of the proposed Committee is two-fold. The first of these would be to undertake any work which may be of use in an advisory capacity or by research, or indeed in any other way for direct assistance in the War. This would, of course, be a temporary object of the Committee, but nevertheless a real one, for, as stated by the Commander-in-Chief at the Front, this is a war of machinery; and a Cabinet Minister, in quoting this, quoted the further statement of Sir John French that it was not the German soldiers our men had to cope with; it was the artillery, ammunitions, and enormously powerful mechanical organisation of the German Army. I need not go into all that may be done in this direction by the Committee, but one step will be to place ourselves at the service of the Ministry of Munitions for such work. A certain number of the proposed Committee may be already doing munition work and also valuable work for other Committees with which they can at any rate keep us in touch; other members would be free to give all their time and service for such work as the opportunity presents itself.

With regard to the second purpose, the matter stands on a rather different footing. We were in many respects quite unprepared for the War on which we have entered, and though this offers one of the most powerful arguments in refutation of the charge that we deliberately entered the War for sinister purposes, it will be very disastrous if we repeat our unpreparedness when the War ceases, and we shall deserve the worst that can happen to us. When peace is concluded, it will only be a prelude to another war, and a war which will recommence with far greater energy on the part of our enemy than before—viz., the war of commerce—and the latter will be almost as serious for us as the more sanguinary one. This will be so even if we are victors, for as an historian (Professor Gardiner) has writen: 'After a long war the difficulties of the victors are often greater than those of the conquered. The conquered have their attention directed to the reparation of losses and are inspired by a patriotic desire to submit to losses for the sake of their country. The victors are in the frame of mind which expects everything to be easy.'

Remembering how soon we forgot that black December week fifteen years ago; and the lurid indication from the German Emperor that he and his people had the will to destroy us then if not the power, and how swiftly

[2] I cannot help paying a tribute to the splendid work of the Economic Section, which Section does not suffer from our peculiar disability.

we relapsed into national ease at the end of the Boer War, it behoves every man who can do so to take his share in making ready for the terrific struggle Germany is certain to put up in the arts and manufactures. I might give evidence of this from a number of sources, but I will only take one emanating from a body of Professors of the great Universities of Germany. These gentlemen have published a voluminous manifesto containing amongst other gems the following: 'Once the Russians are driven back beyond their new Frontier we shall not forget the war which England has made on the maritime and Colonial commerce of Germany. That must be the guide of our action and we must supplant the world trade of Great Britain. By her blockade of Germany, England has instructed us in the art of being a European Power, militarily, industrially independent of others. We must immediately seek to create for ourselves apart from the Empire of the Seas a Continental commercial enceinte as extensive as possible. . . . With regard to war indemnities, we shall demand an indemnity which as much as possible shall cover war expenditure, the repair of damage, and pensions for the disabled men, widows, and orphans. *We know that the question has been examined by the Government according to the financial capacities of our enemies.* From England, which has been so niggardly in men, we can never demand enough money, because England raised the world against us with gold. It is our duty to crush the insatiable cupidity of this nation.' It does not, however, want such published evidence to convince any practical person of the folly of thinking that a keen and virile nation having more than 100 million inhabitants is going to be crushed out of fierce and vengeful competition, whatever the end of the war may be. We shall better appreciate what this competition will mean if we consider the marvellous progress made by Germany during the last half-century in the arts and manufactures. Although we cannot say that this is absolutely measured by progress in the production of iron and steel, or even say that the corresponding rate of increase in production during that period by this country directly measures our progress relatively to Germany, still it does afford some indication in the case of the engineering industry. Probably few of us have ever realised what I can show you by means of graphic curves of production of iron and steel. For these curves I am indebted to the kindness of the Secretary of the Tariff Commission, who was good enough to prepare them specially for my Address, although I do not, of course, put them forward here as a tariff argument. The four diagrams, figs. 1, 2, 3, and 4, represent annual production in ordinates, and the corresponding years by abscissæ, the production of the United Kingdom being shown throughout in full lines and that of Germany by dotted lines. Taking first fig. 1, we have the actual production of pig iron for the last fifty years, and we can see at a glance the much more rapid rate of comparative production in Germany in recent years. When we come to the percentage relatively to the world's production, we see that while Germany is steadily rising, even in comparison with whole world output, the production of the United Kingdom is falling at an even more rapid rate than that of Germany is rising; for whereas fifty years ago it was more than half the production of the whole world, it is now only 13 per cent., whereas Germany's percentage, which fifty years ago was only 10 per cent., has now risen to 25 per cent. of the whole world production.

Figs. 3 and 4 tell the same tale, except that the relative production in steel of this country is now only 10 per cent., Germany keeping for many years the satisfactory figure of a quarter of the whole world's production; and it is of course quite familiar matter that, as far as science and discovery go, Germany owes her growth in steel production largely to the discoveries in this country, although in the case of the Bessemer steel process, she evaded the payment of royalties in making use of the invention. We know that the same tale is to be told in many other industries, such, for instance, as the chemical industry, and we are now suffering severely from the want of the very dyes which were invented by one of our own countrymen.

The above curves are quite sufficient to illustrate the marvellous progress of Germany; and in passing I may remark that one of the most persistent allegations which has been repeated *ad nauseam* by German statesmen, soldiers, professors, and the whole German press generally, is that the War is caused

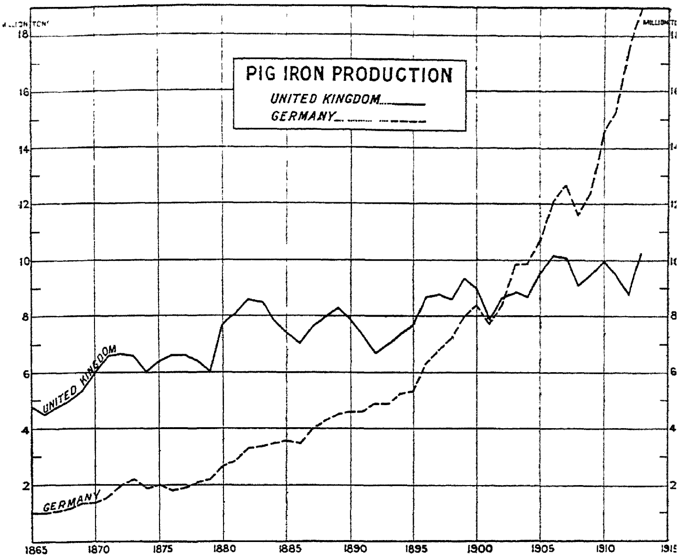

PIG IRON PRODUCTION
UNITED KINGDOM
GERMANY
UNITED KINGDOM
GERMANY
18
16
14
12
10
8
6
4
2
1865
1870
1875
1880
1885
1890
1895
1900
1905
1910

FIG. 1.

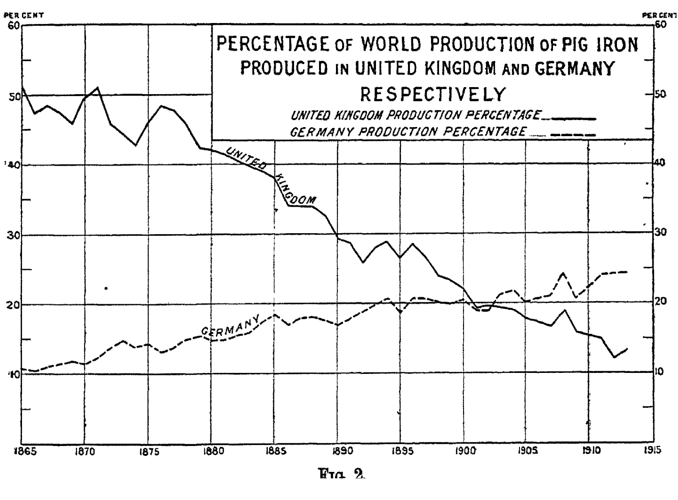

PER CENT
PERCENTAGE OF WORLD PRODUCTION OF PIG IRON PRODUCED IN UNITED KINGDOM AND GERMANY RESPECTIVELY
UNITED KINGDOM PRODUCTION PERCENTAGE
GERMANY PRODUCTION PERCENTAGE
UNITED KINGDOM
GERMANY
60
50
40
30
20
10
1865
1870
1875
1880
1885
1890
1895
1900
1905
1910
1915

FIG. 2.

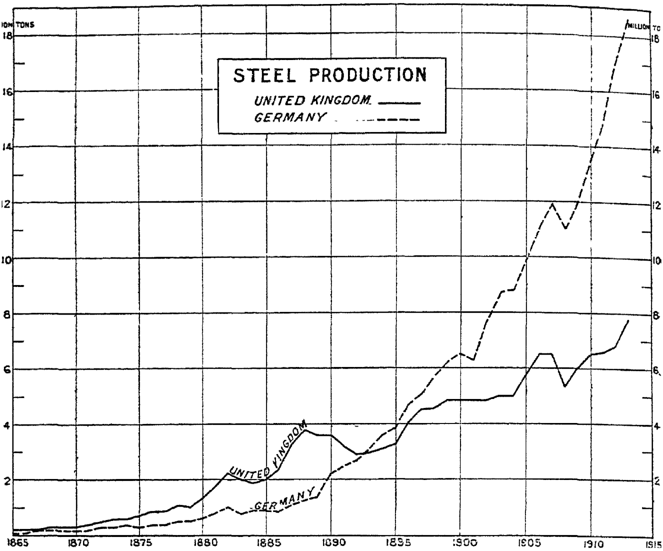

STEEL PRODUCTION
UNITED KINGDOM
GERMANY
UNITED KINGDOM
GERMANY
MILLION TONS
18
16
14
12
10
8
6
4
2
1865
1870
1875
1880
1885
1890
1895
1900
1905
1910
1915

FIG. 3.

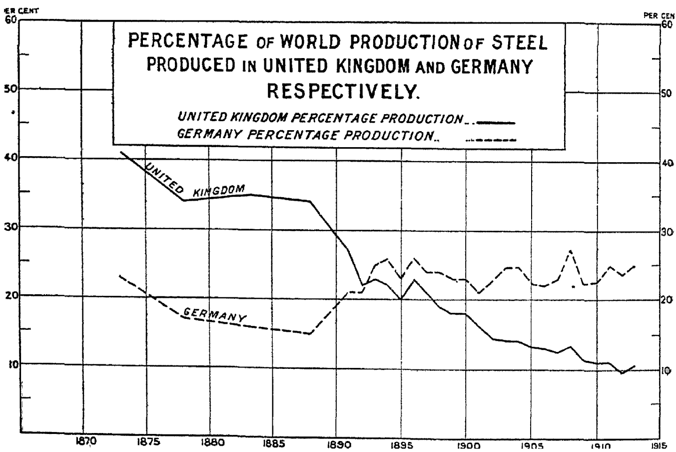

PERCENTAGE OF WORLD PRODUCTION OF STEEL PRODUCED IN UNITED KINGDOM AND GERMANY RESPECTIVELY.
UNITED KINGDOM PERCENTAGE PRODUCTION
GERMANY PERCENTAGE PRODUCTION
UNITED KINGDOM
GERMANY
PER CENT
60
50
40
30
20
10
1870
1875
1880
1885
1890
1895
1900
1905
1910
1915

FIG. 4.

by our jealousy of this progress of Germany. Perhaps you will consider it waste of time to even allude to this matter; but I will take this opportunity of pointing out that if there had been any truth concerning this jealousy, it would have been the simplest thing in the world to shut Germany out of a large number of markets in the British Empire, and that this would have been a very much cheaper process than going to war. Our Colonies, which are now fighting equally with ourselves against German aggression, made a very small difference (5 per cent., for instance, in the case of New South Wales) in regard to the introduction of German manufactures. I myself have somewhat close knowledge of two colonies, and I cannot help recalling the astonishment with which I found in South Africa that, when there was a huge scheme of electrification effected, the enormous amount of material which Germany supplied was for what the public mostly believed to be a purely British enterprise. I also have reason to know that the supply of machinery to New South Wales from Krupp in some cases exceeded by as much as ten times in amount the quantity supplied by British firms. The prices were no higher, in spite of the 5 per cent. advantage to this country. The delivery of the goods was on the one hand sometimes inordinately delayed, though scrupulously punctual in delivery on the other.

Now, when we look closely into the causes of Germany's great advance, we can learn lessons which we have been culpably slow to take to heart. Although there are other causes, first and foremost, and overshadowing all others, the determined and whole-hearted organisation of German industry. I see it recently stated that the scheme above referred to (the Victoria Falls scheme) was lost to this country because the industrial banks of Germany backed their own manufacturers, and this is no doubt partly true. As I have already quoted, Germany's power in war is admitted to be her mechanical organisation, and the organisation of every material and engineering force to that end. Just as striking, if not more so, is her organisation for the arts of peace, and I lately heard a very shrewd man of affairs express his amazement at Germany's entrance into war, when by peacefully pursuing the way she was going she would have dominated the world commercially in a few years' time, and, in the words of the speaker, might in many manufactures have made us practically bankrupt. It is undoubtedly in the matter of scientific organisation even more than the organisation of science that Germany has achieved such wonderful results, and it is therefore in this direction that we must leave no stone unturned if we wish to have any chance of holding our own in the future. I will indicate a few of the matters in which there is ample scope for doing useful work in the above direction.

Education.

A sign of the times is the inclusion of an Education Section in an association for the advancement of science. This has not been done on the narrow ground of improving the teaching of science in schools, but because it is now recognised, and this none too soon, that the whole problem of education must be treated in a scientific manner.

When the subject of engineering education is mentioned we are apt to think only of the training of such engineers as have been considered in a recent report issued by the Institution of Civil Engineers, and to exclude, as that report purposely does, the training of our artisans and foremen. We certainly do not connect the idea at all with the training of the artisan himself. As a matter of fact, while high scientific training of the professional engineer and manufacturer is of vital importance, the proper education of the men whom he will have to control is scarcely less so. The latter education may not be of the same kind, but it is just as vital to the country, and its present condition is a serious evil.

A well-known American, in the 'General Electric Review,' writing on the 'individual and corporate development of industry,' points out that theoretically the aim of both employer and employee is the same, namely, the efficiency of industrial production to increase the return of the investment in labour and in capital. Unfortunately, however, as he remarks, 'the relations between the two have frequently been hostile industrial warfare over the distributions

of the returns rather than co-operation for the increase of financial returns of both parties.'

One of the most humiliating things of the present War has been the mutual relation of the two in this country in what is probably the most critical period in our history. I will say more later on this subject, but there is no doubt the subject of industrial education needs earnest consideration. Take the first, the education of the professional man—the class, for instance, joining such institutions as the Civil, Electrical, or Mechanical Engineers; we find in one respect a most satisfactory progress as to what is insisted upon before such men are allowed to join one of these bodies. All such institutions now demand technical diplomas or University degrees, and, in addition, satisfactory evidence of practical training. But to prevent injustice to the man who may be self-taught, they hold examinations conducted by recognised men of standing in scientific and technical subjects. Great as this progress has been in recent years, there is a great deal to be done. In the first place, professors and teachers of engineering and technical subjects have to deplore the miserable previous training of a large number of students. It seems still to be a common idea that if a boy is unable to make any decent progress in the usual school subjects he can be sent to a technical school if he is useful with his hands, under the pathetic impression that the success of the engineer depends upon his hands rather than upon his head; and the first year or two at a technical school or college is thus taken up with work that ought to be done at a secondary school. It is not fair to put all the blame upon the school, for I have known students coming from the classical side of such a school with no knowledge of science and very little of mathematics who have taken the highest places in the engineering course, and, after entering their practical work, rapidly risen in the profession, and this was because such men had been well trained to apply their mind to any subject and had a sound foundation upon which to build. This only shows that a good student will always rise to the top, and does not prove that the present school system is the best for an average boy or makes him work as hard as, for instance, the corresponding school in Germany does. A large number of thinking men are convinced that our whole education system seriously needs reform. I say this not merely in reference to scientific education and technical training, but to the whole attitude of mind of the young of all classes of the community towards the serious work of life when they leave school. I will allude to this under another heading later on. In the matter of education and its bearing upon technical training, we have, then, a good deal to learn from Germany. There are some things that we think are quite as good if not better in this country, but there is no reason why we should not try to find a way to adopt the better features of education from our enemy, and, while retaining independence of thought and originality, inculcate firmer discipline, for there is surely a happier medium in this matter.

There is one matter before passing from this subject which calls for remark. I see in the report above alluded to there is a great divergence of opinion concerning the wage-earning value of highly technical students. Here again is a matter which in itself is worth a very careful discussion. The question depends first upon the student himself, next upon the kind of training he has had, and then upon the nature of the work he is expected to do. The blame in not getting the best results from a well-trained student is very often due to the employer, and our Section might do something to bring professors and employers into closer touch, and both employer and professor may have something to learn from each other.

In leaving this subject I cannot help pointing out what important continuation schools are to be found in the meetings and discussions of the younger members of various engineering Societies and how much a young engineer learns in the preparation of a paper. Anyone who is accustomed to take the chair at such meetings will bear witness to the excellent outlay of money represented by the award of prizes and medals for such work. Many men to my knowledge have got jobs through thus showing acquaintance with a special subject or originality of thought.

Research.

If there is one thing more than another which the British Association can be congratulated upon, it is the work which it has done in the matter of research, and it is very interesting to go back to the earliest days, more than eighty years ago, and to see how, in very different days from the present, research in all branches of science was encouraged, and what a potent factor the various meetings have been, not only in actually fostering the work of research itself, but in obtaining the recognition which is accorded to-day. Amongst other things, the National Physical Laboratory stands largely to its credit, as having been first powerfully advocated at one of its meetings. This Section has not been behind the others, and at the present moment there are three Research Committees, viz., those on Gaseous Explosions, Compound Stress, and Impact. The work of the first of these is so valuable that its results have been published all over the world.

To-day there is a more general recognition of the importance of research, and the recent institution by the Government of a Committee for the organisation and development of scientific and industrial research is the latest indication that the nation is beginning to realise its importance.

So far from all this making our work less necessary, there is all the more reason why we should have a permanent Committee of Research, because one of the intentions of the new Government Committee is to utilise the most effective institutions and investigators available, and the statement is made that one of the objects of the Government Research Committee is to select and co-ordinate rather than originate, and that one of its chief functions will be the prevention of overlapping between institutions and individuals engaged in research. The Government Committee in question is only dealing with the organisation in England, Wales, Scotland, and Ireland. Now, the great advantage possessed by this Association is the fact that it includes not only Great Britain and Ireland, but all the Colonies, and indeed one of the three researches above mentioned is being carried out in Australia. Another research of the Association is being carried out in Cyprus; and work is also being done in such places as Jamaica and Egypt. It is more important therefore than ever that the British Association work in research should go on, as, since its members are drawn from all parts of the British Empire, its influence should be correspondingly great.

There is another reason for research being a subject of a permanent subcommittee, and that is that suggestions for new work are more likely to be matured, and work of an advisory nature made more practical than is possible at one annual meeting.

There is yet one more reason, which is that, although we have made some progress, we are still far behind Germany in the organisation of research. There is no doubt that our students and scientific men are quite capable of conducting researches, but the training for this is like the training for the officers of an army: it cannot be done hastily; and, indeed, men themselves cannot be obtained for this purpose without years of preparation. All such work must be done as a factor in the reorganisation of our manufacturing and commercial resources in the great struggle that lies before us.

There is one subject which affects both education and research, and might be a matter to be reported on by our Committee. It is very rarely that a professor is both a good teacher and gifted with the power of original research. Even when a professor or lecturer is so gifted, however, it is almost impossible for a man to really devote himself properly to research, and at the same time undertake the duties which are attached to a professorial chair. Why not face this subject boldly, even relieve the bad lecturer (there *are* men who admit their failure in this respect) of a certain amount of his work, provided he is doing well in research; or for the man who can do both well, see that he not only has efficient assistance, but even more, that he is given the opportunity of devoting long periods (for instance, alternate years) entirely to research.

There are numerous other questions which would come up under this heading, and which could be usefully dealt with by our Committee.

There is one more subject that we might consider, and that is a better differentiation of researches on purely industrial work, such as are often of a most

profitable nature to the professor or research student, and those which are of a purely scientific character. While it is only right that every successful research, even if conducted at the expense of a public body, should bring solid return as well as fame to the worker, some steps should be taken as to the fair and equitable distribution of the proceeds. I see that one of the proposals of the new Research Committee is that discoveries by institutions, associations, bodies or individuals in the course of researches aided by public money shall be 'made available under proper conditions for the public advantage.' If the discovery is patentable, I assume, it would be protected at home and abroad, unless we wish to spend public funds as much for the benefit of foreign trade rivals as for ourselves. This is one of the many matters in connection with which a British Association Committee might from its cosmopolitan character render great service.

Standardisation and the Metric and Decimal System.

One of the favourite jibes at this country is our supposed utter want of system in regard to our standards and systems of measurement generally. With regard, for instance, to the decimal system, it is frequently stated that thirty or forty countries have adopted the metric system, while only three retain the inch as a standard. It must be remembered, however, that the population and wealth of the three latter are at least equal to, if not greater than, all the others, though this does really not prove anything, except the difficulty of the subject, and that there is a great deal to be said for both sides. In the Report of the Decimal Association last April, the hope is expressed that one of the changes for the better arising from the War will be a reform of our weights and measures. No class of the community would be affected more closely than the engineer, and engineers cannot fail to be interested in the question as to whether the general and immediate adoption of the metric system would or would not be a valuable means of assisting British firms in their competition with Germany and Austria, in countries where that system is in vogue. Although it is very unlikely that a wholesale change is imminent, it is certain that the metric system is gradually spreading, and in the United States and Australia very strong forces are on foot to bring about a change to that system. The British Association has over and over again had the subject before it, and our Committee might be of service in making a report on the present state of the matter.

One thing is certain: the Committee might be of assistance in recommendations which would bring into line all British engineers in duplicating tenders for countries which have the metric system.

Coming to standardisation, here we have more ground for satisfaction. The Standards Engineering Committee during the last ten years has done a work which is quite equal to that in any other country, of completing standardisation of all important matters in engineering, and, moreover, has secured the recognition of these standards in all public contracts. As giving some indication of the range of this work it may be said that there are more than sixty committees for dealing with every conceivable engineering matter, from bridges, ships, and locomotives, down to electric lamps. One of the last of these committees, dealing with the automobiles, has eleven sub-committees, many of which have already completed their work. It is almost impossible to do justice to the extraordinary achievement of bringing order out of what was apparently hopeless chaos and to the benefit of the British engineering industry of this work, largely due to the energetic secretary, Mr. Leslie Robertson. We may justly pride ourselves that this Section was a pioneer of standardisation by taking up the subject of small screws, its work being taken over ultimately by the Standardisation Committee.

There is yet work to be done, however, and one matter of great importance would be to get a universal standard of temperature for instruments of measurement other than zero. A temperature, for instance, of about 62° Fahr. would make steel rods' measures more practically workable than at present.

In connection with the subject of temperature and standardisation, I recently came across a statement by the General Secretary of the International Electrotechnical Commission (Journal, Jan. 1915) that the want of uniformity in the rating and testing of electrical machinery has been a serious evil, and

he goes on to say: 'The German standardisation rules, for instance, which, through well-organised and combined effort on the part of the German makers, had previous to the War become widely recognised on the Continent of Europe as well as in many countries to which British machinery is exported, by permitting a higher temperature rise than is considered good technical practice in Great Britain, certainly have not assisted the British maker in foreign markets.'

Exhibitions and Museums.

In recent years a large number of commercial exhibitions have been held of all branches of machinery, and it is satisfactory to note that one of the features of such exhibitions has been the holding of scientific lectures, and the inclusion of the exhibition of scientific instruments and apparatus, and also exhibits showing the relation of scientific experiments to engineering work. In some of the privately organised exhibitions with which I have been associated myself, the scientific men have been invited to take part when the general lines had been settled on which the exhibition was to be run, and thus we had comparatively little influence. I have thought from time to time that it would be well if a permanent committee of such a body as the British Association existed, which could exert more direct influence, chiefly of course by reports and recommendations. The managers and organisers of such exhibitions would value assistance of this kind, and in return would listen to suggestions which might materially add to the scientific value of such an exhibition. I know from experience that a British exhibition is a most important means of promoting British industry, for the number of inquiries that come from all parts of the country and from all parts of the world show how much interest well-organised exhibits arouse and what long distances people will travel to attend such an exhibition. A machinery exhibition was to have been held in London, the date of the opening a week or two after the date at which the War began, but was of course not held. This exhibition was to have been Anglo-Dutch, and though organised by private enterprise was even in advance bringing in touch the consumers and manufacturers of the two countries. The *Beama Journal* quoted recently an American magazine in which the writer was advocating the support of a permanent Commercial Museum for industrial purposes, and this is what he said: 'We produce a surplus of manufactures that must be sold. Our manufactured exports have about doubled in ten years—in truth a cause for satisfaction, and yet we have not accomplished enough. . . . We have only made a beginning, considering what we can do and will be forced to do in the future. . . . Manufacturers must compete with old-established nations in the market they seek to invade.' It is noteworthy that this museum, which is really a permanent exhibition, is a very complete organisation, containing amongst other things science laboratories.

It is sad to think that the great hopes held out of the Imperial Institute by the President of this Associaion, Sir Frederick Abel, at the meeting held in Leeds in 1890, have not been altogether fulfilled. The President expressed the belief that amongst other objects the Institute would combine 'the continuous elaboration of systematic measures tending to stimulate progress in trades and handicrafts, and to foster the spirit of emulation amongst the artisan and industrial classes.' It may be a very fitting time to bring forward the whole question, because it has often happened that an excellent scheme which has somewhat languished has upon its revival at a later time when its importance was better realised been crowned with success.

Another matter which might be considered is the question of departmental museums at the Technical Schools and Universities throughout the country. The organisation of these is simply a matter for the enterprise of the individual professor in each department. The museums in the Engineering Department of the colleges with which I have been associated were very much appreciated by the students, who constantly were the means of securing fresh specimens, and after they have left the college continued to contribute articles of great interest, such as fractures, corrosions, boiler plates, models, &c. This matter might be handled in a much more systematic manner, and possibly a report from our Committee with a recommendation to the proper quarters would be of use.

Patents and Patent Laws.

This subject is well worthy of the consideration of the proposed committee since progress in Engineering, certainly on the mechanical and electrical sides, is largely dependent upon Invention, which is not likely to be seriously undertaken without adequate protection, not entirely for the inventor, but also for those who really make the invention practical by means of capital and business support. A great deal of nonsense is talked and written about inventors, as if they were a special class of being, generally mad and always impossible. Some inventors are both, but the fact is, most engineers spend their lives seeking new ideas and devising new methods of carrying them out, in short, in inventing. It is of the greatest importance that every step should be taken to encourage sound invention and to see that anything of value is secured for this country. Of course, every invention worth anything is immediately known in other countries, but I need not argue to this Section that the country which actually produces the inventions is at a great advantage quite apart from the royalties payable on foreign patents. The foundation of the Munitions Invention Panel is a step in the right direction and will doubtless be followed later on by Government Committees for peace inventions. Such Committees or Government Departments dealing with various industries will be assisted by suggestions from a body like this. Take, for instance, the present state of Colonial patents; within the last few years one Commonwealth patent has been made to cover the whole of Australia, instead of there being, as of old, separate patents with different regulations and fees for each separate Colony. South Africa has not yet conferred a similar boon upon inventors, and we might do something to expedite this desirable innovation. But this touches the much wider question of Colonial Patent Laws as a whole. These are all different and differ from those of the Mother Country. It would be a splendid thing if we could bring about a conference leading to unification of these diverse Patent Laws and have one comprehensive Patent Law for the whole Empire.

There are many other matters, for instance, the question of extending the time of secrecy in the provisional patent. The 'close' time in patents was the act of Chamberlain, and is a splendid legacy of that great man, but for really many important patents the close time allowed is not enough.

Another is one in which the German system has certain advantages, viz., in having two classes of patents. One of these is the patent 'proper,' which is only granted after the most severe search and criticism and holds the usual period when granted. The other is a secondary patent granted for the shorter term of five years, and is given for one of the hundred and one minor improvements and devices which, though of real value, only constitute small modifications in detail and not new applications of principle.

Having previously spoken of German ways pretty plainly I should like to say here that I believe the suspicion of injustice to the British and other foreign applicants by the German Patent Office is to a great extent if not altogether unfounded. It is doubtless true that German manufacturers in common with most of us would like to avoid paying royalties, and it is moreover a common belief in this country that there is an advisory committee of manufacturers associated with the German Patent Office. This view is supported by such statements as in the prospectus of the Deutsche Maschinenfabrik, which runs as follows: 'With the present-day competition every firm is compelled to protect its new designs by means of patents, *and watch that no other patents are granted* which would seriously effect (*sic* affect) it.' Notwithstanding an utterance like this, my own experience and that of others is, that if the stringent rules of the German system are observed any valid application is granted, the motto which appears to guide the officials being, 'We will be just, but we cannot afford to be generous.'

There are other matters, such as the question of giving wider powers to our Comptroller to refuse a grant where novelty is less than microscopic. Here again the German system of demanding that some definite principle is applied *to produce some definitely new effect*, might to some extent be followed, especially in view of the constant accumulation of published devices, some patented and others not.

I will conclude this section, which is far from exhaustive, by pointing out what a debt of gratitude Engineers and others owe to the Patent Office for the manner in which the work of producing illustrated abstracts of all patents has been and is being done, and the weekly issue of the 'Patent Journal,' but this may be associated with the suggestion that it would be a real convenience if, instead of the delay which often occurs, the abstract appeared at the same time or immediately after the publication of the complete specification.

Organisation.

This, I venture to think, is by far the most important question of any I have raised, and I will go so far as to say that I believe it to be the all-important one, as it practically embraces the others. If you do not agree with me, I feel sure it is because we do not understand the same thing by the word 'organisation.' When you speak of organisation to most people they immediately seize upon some small feature which may be to them of more immediate interest. It may be the general arrangement of their accounts, their system of store keeping, of dealing with their workmen, of the sales department, or fifty other minor details. If you take this narrow view of organisation you will, of course, at once say that a scientific man has very little to do with it, and indeed the manfacturer as a rule, thinking of his works organisation, scouts the idea that a man of science can either know or have anything to say about it which is of any value.

Let me therefore take the dictionary definition. To organise is to 'arrange or constitute interdependent parts, each having a special function, act, office, or relation with respect to the whole.' If we accept this definition, which as a matter of fact we must, there is no question as to the all-important nature of organisation, for you will notice there are two outstanding things. The first 'interdependent parts"; and the second their 'relation to the whole.' Thus the subject of organisation really includes the whole of industry. It includes science and its relation to manufacture. It includes the relations between the employers and employee. It includes the workman, and his attitude towards new devices, labour-saving appliances, and output. It includes the whole question of the supply of raw materials, and even the sale and delivery of the finished article. Taking these different features, is there any doubt that the man of science in this country can hold his own, and more than hold his own, with that of any other? The history of invention is quite enough to give a final answer to this question. Again, the British employer and man of affairs has always shown himself individually in the forefront of enterprise; as for the workman himself, he is admitted, in the matter of intelligence, physical endurance, and skill, to have no superior; while with regard to materials for manufacture, and the power of delivering goods, it need scarcely be said that the British Empire, if we take is as a whole, is the richest country in the world in raw materials, and its means of delivery of its goods is expressed by the enormous preponderance of its mercantile marine.

When we come, however, to these interdependent parts and their relation to the whole, it is there that we find the weak joint in the armour. It is in this respect that Germany can teach us a striking lesson in the *arrangement* of these interdependent parts with respect to the whole. From the top to the bottom the whole forces of their industries are so thoroughly organised that they get all that is humanly possible out of the various factors. I do not limit this merely to the wonderful organisation of any works, like Krupps, or the Deutsche Maschinenfabrik, or hundreds of other works, but I include the organisation of all the Government Departments, together with the Banks, the Railways, and the Shipping, so that every facility is afforded for the world commerce of the German Empire.

Taking only one of these details, I remember, when at Liverpool, and the battle of the Manchester Ship Canal was being fought, what facts came out as to the difficulties in the transhipment and handling of goods. The late Mr. Alfred Holt, for instance, was one of the most earnest in pointing out that the want of co-operation and organisation in getting goods from our manufacturing centres was adding largely to their cost, and actually exceeded the cost of transporting these goods across the ocean. In Germany, on the other hand,

the Government steps in, and by means of special differential rates, gives the manufacturer every facility, and the lowest possible rates for obtaining raw material, and delivering the finished goods to all parts of the world. It was this organisation that not only rendered Germany so formidable a rival in times of peace, but makes her so powerful in war.

This co-ordination in Germany is carried out in every industry in a way we generally have little idea of. For instance, the other day at a deputation to the Government Mr. Runciman remarked that the difficulty of connecting the manufacturers with the commercial staffs in this country is deep seated, but perhaps not altogether incurable. Further, that the manufacturer must realise what he can get from the universities, and the University must know what the works require. Dr. Foster, the Treasurer of the Chemical Society, also said that 'the Germans were so imbued with the need of pursuing modern and efficient methods of education, in applying science to industry, that they hold in contempt a country which notoriously neglects such processes'; and he attributed this contempt as partly contributory to their cheerfulness in entering into the war with us.

Now while these remarks are undoubtedly true, they are only a part of the truth. The evil is far wider than in any special application, for, as the German knows perfectly well, there are innumerable individual cases of organisation in this country of equal efficiency to any in his country, and he is glad enough to learn from special cases. Let us take one, and I do so because it shows that the man of science is capable of industrial and manufacturing organisation, if he turns his mind to it. I refer to the case of the firm known as Barr and Stroud, Ltd. As you know, the founders of this firm were originally colleagues in the Yorkshire College (the former, Professor Barr, occupied the Presidential chair of this Section three years ago), and they together invented a range-finder. Now, whatever the merit of this range-finder, it is safe to say, like every other important invention—for instance, the Parsons turbine—that the invention alone would have stood a small chance of coming into practice. In fact, to make the invention is, as a rule, the beginning of the difficulty. Professors Barr and Stroud, however, set to work to carry their invention into practice, and did so with such effect that their works, which began on quite a small scale, rapidly grew, and the first part of the new works was opened with about 90 hands all told in 1904. In the course of ten years it has increased to such an extent that there are now 1,700 employees. Those of us who have visited the works at Glasgow know the almost perfect way in which the whole arrangements are made, not merely for the scientific side, but for the comforts of the men, including the working dress which in itself becomes a uniform. It gives some idea of the scientific side to know that there are at the present moment twenty-three men with high university qualifications, most of them with university degrees, and many of them men who were absolutely the first on the college list in the final examinations. This industry is another illustration of the lead given to Germany by this country, because the Barr and Stroud range-finders were brought out before any of the German range-finders of the kind now being made, the Germans having followed in their lines, and copied them in many respects. I have enlarged upon this, because I cannot help pointing out that the Barr and Stroud range-finders have had no small effect in the marvellous precision of our naval guns, and it will no doubt pass through your minds what we owe to private enterprise which started the manufactures of the turbines, range-finders, guns, and other naval features, when we think of such battles as those off Heligoland or the Falkland Islands.

Now I do not believe the Germans despise us for our want *per se* of the application of science to industry. I do not think they have much reason to; but what they do despise us for is the want of co-ordination, which I venture to say amounts to positive slackness, which they are keen enough to observe permeating the whole of this country. They see different sections, instead of being united for a common end, quarrelling with each other, filled with mutual suspicion and distrust, with apparently no common bond of union, and whereas the German is proud of the Fatherland, he sees in this country large numbers who seem, either through self-consciousness or ignorance, to be ashamed to mention the subject of the British Empire, or what is worse, to acknowledge

that any love of their country is or could be a mainspring and incentive to strenuous effort.

The other day, Field Marshal von Moltke stated, and there is no reason to disbelieve him, that great as was the storage of ammunition and shells before the war, the enormous demand far exceeded all expectation, and Germany found herself for a time in the same plight as her enemies, but he further stated that Germany's emergence 'from this dangerous position was largely due to the extraordinary organisation, which included not merely the adaptation of their factories for munition purposes, but *capacity for work of the people,* and the *patriotic spirit of the German workmen.*'

This brings me to consider what is probably the most serious feature in our national life to-day, which I have already alluded to under the heading of Education, viz., the relation of employer and workman. It is hopeless, as long as such ideas prevail which seem to do at present, to think of any sound organisation of our industrial system taking place, because the interdependent parts are not arranged (and can never be arranged until we change radically) with respect to the whole. Now as one who has served an apprenticeship, who has taken his money weekly from a tin box with hundreds of other men, who has been a member of the Amalgamated Society of Engineers (in fact was working as an engine fitter when a Whitworth scholarship made a college career possible), I am the last man to put this evil down entirely to the working man. I know individually he is just as capable of patriotism as any other class. Get him by himself, even the men whose strikes have caused such despondency in the minds of our Allies, and who have seriously jeopardised the very existence of the country, and you will find (except in the sort of case to be found in all classes of society), that he, as an individual, is willing to make sacrifices, and if necessary to give himself for his country. The truth is that the canker which is eating the heart out of our industrial life is due to an entirely wrong attitude of mind. For instance, however much we may sympathise with men who see a loss of employment in the introduction of labour saving machines, some means should be found by which they can share the benefits to the State and to their employers by the introduction of such machines. I should like, if I had time, to say something about the marvellous organisation of the Ford motor-car works in America, and how it has given the men a share in the returns of a great industry, and thereby induced them to work in a way that has enriched themselves, their employers, and their country. We have many splendid examples of this co-operation in this country. For instance, Messrs. Allen, of Bedford. Again, the employment of women in the engineering industries has taken place in many directions owing to the war. The works with which I am associated could not have undertaken much munition work without it. Some steps should be devised by which this avenue of industry is not closed to women after the war, while justice is secured for the men alongside of whom they are working, and from whom they are in many instances learning mechanical skill. Again, the questions of piecework and overtime must be seriously considered by the State, and not allowed to become the subject of disastrous disputes. Once more there is the question of a standard wage. It is against the eternal laws of nature to try and keep living beings at one dead level of equality and merit—*i.e.*, it is against the law of the survival of the fittest. The trade unions have a great opportunity of placing their country and themselves in a leading position amongst nations if they will courageously grapple with a great problem by recognising degrees of merit and corresponding degrees of payment. These are a few of the many matters which must be dealt with in the immediate future.

The matter of labour disputes is so serious as to demand plain speaking. It must be admitted that there are many employers and companies which, to satisfy themselves and their shareholders, extort the largest possible dividends and pay the smallest possible rate of wages, and do so apparently without the slightest idea that the men and boys under them are capable of education and personal influence. Can it be wondered then that men under these conditions are willing enough to listen to the orator who merely appeals to their fighting instincts and join in the game of grab as against the employer. On the other hand, strikes have occurred when employers have honourably carried out their

obligations and undertakings, and the men have shamefully departed from an agreement made by their chosen leaders, throwing over the leaders the moment they have fancied it to their own selfish interests to do so and without a single thought of their duty to the community as a whole.

We have recently seen the Prime Minister and other leading statesmen struggling, sometimes in vain, to bring large bodies of men to a reasonable state of mind. Is not this (and I speak without the slightest reference to party questions) a case of nemesis overtaking us for having in so many cases pandered to the selfish instincts of large bodies of men in order to secure their votes, instead of sternly telling them unpalatable truths?

There was recently an intensely interesting article by the late Professor Friedrich Paulson, previously Professor of Philosophy in Berlin University, published in the 'Educational Review' of New York. In this article, the subject of which was 'old and new fashioned notions about education,' he pointed out that the whole of our educational system was going wrong, and that we could not escape the conviction that a tendency towards weakness and effeminacy was its chief trait. His three mottoes were: learn to obey; learn to apply yourself; learn to repress and overcome desires; and he remarked with great truth under the first heading: 'He who has not learned to do this in childhood will have great difficulty in learning it in later life; he will rarely get beyond the deplorable and unhappy state that vacillates between outward submission and uproarious rebellion.'

Is not one of the first things the reform of our educational system?

The other day a writer in the *Spectator* said with great truth that 'what Great Britain is suffering from acutely and dangerously at the present time is the absence of discipline,' and a neutral writer in the *Times* remarked as follows: 'The uniformity of German effort, due doubtless to their myriad well organised, machine-like minds, though it renders them excessively tiresome people to dwell among in peace time, enables their Government to extract every ounce of energy in the conduct of a war.' He further went on to say that the British Empire 'could not have been created by minds like these, but it should not be forgotten that in the concentration necessary to national effort in a struggle like this the German system of self-subservience to the State has enormous advantages.'

One of the tasks to which the British Association might bend its energies with the greatest benefit to the country, is to bring about a reform of our educational system, so that while we do not kill individual enterprise and freedom of thought, which have contributed so largely to the political organisation and constitution of the British Empire, of the value of which we have had such wonderful evidence from our colonies and dependencies during this war, we seek to implant in the minds of young and old those ideas of discipline and service to the State, the want of which so seriously threatens the successful organisation of our industrial life.

Conclusion.

In bringing my address to a close I hope I have made it clear that I have had throughout a practical object. Expressed briefly, it is that the service of every agency is wanted for definite work at this crisis, both in the actual war, and afterwards in the war of industry which will be waged with equal intensity in peace time. The British Association cannot be said to have undertaken as a whole a work of this kind, yet one finds a general desire on the part of every member that something should be done. With this object I communicated with the President, and found that both he and such of the officers as could be got in touch with were in entire sympathy with the general proposal, and advised that our section, like that of Economics, should start at once with a committee on the subject. I have great hopes that such a committee will be formed, but I have no hopes of either our own sub-committee or the committee of the Association as a whole doing any good, unless they are prepared with definite suggestions and advice which cannot be ignored and put aside. I have not the slightest faith in the mere formation of a committee which will content itself, let us say, with the mere offer of its services, even to a Government department, and the mere pious expression of certain

opinions. If a committee does not want to become ridiculous, it must show that it is in earnest. To show that it is in earnest it must take care that its reports have a practical object, can be at once grasped by overworked Ministers and officials, and are of real value. Of course there are incompetent people in public departments, possibly even in the Admiralty and War Office, and many good proposals and suggestions are turned down—or, let us rather say, have been turned down in the past—because they happened to pass into the hands of such people in the first place, and there was not enough driving force behind them to follow the matter up.

When I first used to attend meetings of the British Association there was a gallant officer (Captain Bedford Pim) who had commanded various men-of-war, and was patriotically concerned with the state of the British Navy. I remember well his formula, which I heard on many occasions, as follows: 'The British Navy, sir, rotten—rotten from stem to stern, from truck to keel.' Such a sweeping statement about a service of which we are all proud only served to raise a prejudice against him, in which I shared myself, and excited the suspicion of undue bias or twist of mind. As a matter of fact, as it turned out afterwards, and has since been admitted over and over again, he was essentially right, and now that we realise our obligations to the British Navy, and that it has really saved this country, one trembles to think what would have happened if it had then been called upon in the same way as in these days. The above officer was afterwards made an admiral, though I am afraid it was not as a reward for his candour, or even to head off his criticism, because nobody seemed to take much notice of his warning. The moral that I have in mind is that if our committee is going to be of the slightest service, while formulating its proposals in temperate language, it must unflinchingly follow them up, and not allow them to die unless they are proved to be worthless, but to see they are seriously taken up and carried into operation.

Fortunately the British Association is a powerful body with great traditions, and will be listened to if such work is carefully and energetically done. Think, for instance, there are many eminent men who have supported this particular Section in times past, and many of them in the chair, such as Robert Stephenson, Scott Russell, Lardner, Moseley, Willis, Whewell, Whitworth, Vignoles, Fairbairn, Rankine, Hodgkinson, Sopwith, Babbage, Hawksley, Hawkshaw, Barlow, Armstrong, Froude, Bramwell, Baker, Douglas, Osbourne Reynolds, White, and many others. Think of the mark that these men, now passed away, have left on the history of the British Empire, and let us see to it that this Section does something worthy of its past history. We can at least congratulate ourselves that whatever the evils of the War, the country as a whole has been moved from its usual attitude of self-complacency, and that the numerous new departments and organisations are showing a desire to utilise every force and agency for the service of the State, and to grapple with the great problem of its more efficient organisation. It will be no small work of a British Association committee if it can supply sound ideas and recommendations on the many thorny problems which must be solved. We cannot all of us be, as so many would like, in the fighting line, either in France or the Dardanelles, but we shall be just as deserving of contempt as those who, having had the opportunity of service, have shirked their responsibilities, or the giving up of their sons, and are even thinking of the War as a matter of personal gain, either in purse or reputation, if we content ourselves with mere offers of service, and having as we think shelved responsibility by leaving initiative to others, we pass along our way sheltering ignobly behind those men and women who are doing their duty to their country.

British Association for the Advancement of Science.

SECTION H: MANCHESTER, 1915.

ADDRESS

TO THE

ANTHROPOLOGICAL SECTION

BY

PROFESSOR CHARLES G. SELIGMAN, M.D.,

PRESIDENT OF THE SECTION.

It is impossible to pass to the subject of my address without first referring to the heavy losses which the Teutonic lust of power has inflicted upon our science, no less than upon every other department of humane and beneficent activity. Whatever loss we may yet be called upon to endure there can hardly be any more regrettable than the death of Joseph Déchelette, whose acknowledged eminence makes any detailed account of his labours superfluous. I will but mention his *Manuel d'Archéologie*, a work of rare lucidity, which though unfinished (the pity of it) will long be authoritative upon European prehistory and archæology. His valour was no less than his erudition, for though his age exempted him from all military duties, he insisted on taking his place at the head of his old company of Territorials, and was killed last October while leading his men in a charge that carried the line forward 300 yards. How he died may be learnt from the official army order quoted in *L'Anthropologie* (vol. 25, p. 581). We have also to mourn the death of Robert Hertz, a regular contributor to *L'Année Sociologique*, and of Jean Maspero, son of Sir Gaston Maspero, an authority on the Byzantine period and Arabic geography.

The other men whose premature death we deplore belong for the most part to that brilliant band of French soldier-explorers to which African ethnography owes so much. Captain René Avelot, whose name will be known to every reader of *L'Anthropologie*, was also the author of important papers in *La Géographie* and other geographical periodicals. He had hoped to devote himself entirely to ethnological work, and at the outbreak of war was about to publish a series of studies on the natives of the French Congo, Darfur, and Wadai. Before going to the front he arranged for the publication of these, even in the event of his death.

Captain Morice Cortier, a geographer and an explorer rather than an ethnologist, was engaged at the time of his death in preparing a work on the prehistory of the Sahara.

Captain Maurice Bourlon received his scientific training in the Dordogne. He conducted excavations in the neighbourhood of Les Eyzies where he made a number of discoveries, and brought to light some remarkable specimens of palæolithic art.

We dare not hope that the foregoing list is final, and while mourning their loss we cannot pay greater honour to their memory than by taking up the burden they relinquished in the hour of their country's supreme need.

In my address I shall endeavour to outline the early history of the Anglo-Egyptian Sudan from the standpoint of the ethnologist, and thus indicate some of the lines upon which future research may most usefully proceed. Fortunately the Sudan is one of those areas in which the site and scope of work may be selected from the point of view of immediate scientific interest, without any grave dereliction of duty. There is not the danger so frequently met with, in the

Pacific for example, of finding that civilisation has come stealthily and swept away the greater part of the old order. The Sudan has not been civilised to such a point that the ethnologist need feel it his bounden duty to visit the most sophisticated areas in order to record everything that is in danger of being lost. If I seem overbold in trying to present a summary account of so vast an area, I can plead certain extenuating circumstances. Firstly, I had the good fortune to be asked by the Sudan Government to conduct a small ethnographic survey for them, in the course of which I have spent two winters in the field; secondly, although it is nearly twenty years since the reconquest of the Sudan, the amount of ethnographical material which has accumulated is far less than might have been expected. Nowhere has there been any intensive study of even a single people, though conditions are as favourable as could be desired in at least three quarters of the country. It would be an interesting, if melancholy task to try to determine why so little has been done. Certainly the failure is not due to lack of interest on the part of the Sirdar and his Council, for apart from the funds provided for my expeditions the Government has paid the expenses of publishing the only considerable work of serious scientific interest dealing with Sudanese tribes that has yet been written by any of its officers. It is worth noting that archæologists have been more energetic than ethnologists, partly no doubt owing to the stimulus provided by the discoveries of the Nubian Archæological Survey, but even apart from this more interest seems to be taken in the ancient history of the country than in its living races.

Surprisingly little is yet known of the prehistory of the Anglo-Egyptian Sudan. No implement of river drift type appears to have been found, and while admitting that this may be due to incomplete exploration, the fact seems of some significance considering the abundance of specimens of this type which have been found on the surface in Egypt, Southern Tunisia and South Africa. With regard to implements of Le Moustier type, I may allude to certain specimens which I have myself collected from two sites, namely from Beraeis in north-west Kordofan, situated on a sandy plateau at the foot of Jebel Katul between two small spurs of the main rock mass, and from Jebel Gule in Dar Fung. At the former site I found a number of roughly worked unpolished stones near the foot of the hill between the village and the burial ground, and also within the latter. The majority are moderately thin broad flakes, showing a well-marked bulb of percussion, and little or no secondary working; other specimens are shorter and stouter. One surface is flat and unworked, the opposite curved surface shows a number of facets separated by rather prominent crests, all except the central facets sloping more or less steeply to the working edge. In some specimens the crests are sufficiently prominent to give a somewhat fluted aspect to the slope and a crenelated edge, one portion of which often shows signs of having been worn down and retouched. These implements, which I had suspected might have been Aurignacian, were considered by M. Breuil to belong to the Moustierian period, and he referred to the same period and industry some thick, fluted and engrailed scrapers from Jebel Gule, which I have described as resembling the palæolithic discs from Suffolk and other localities, (**19**, **211**), as well as some implements of other forms which presented a palæolithic facies. While the Beraeis stones are so rough that they may perhaps be rejects, this does not apply to the specimens from Jebel Gule. Besides the disc and Moustierian points there is one implement which M. Breuil regards as a true, but much worn, *coup-de-poing* of Moustierian age. Whether all these really date from the Moustierian period or not, certain of the specimens from Jebel Gule show a surprising resemblance to South African specimens figured and described by Dr. L. Peringuey as of Aurignacian type (**14** Pls. xvi. $\frac{130}{5}$, xi. 60, xviii $\frac{132}{3}$), or in other words of the Capsien type of Tunisia.

Evidence concerning the later Stone Age is furnished by a number of finds made on widely scattered sites; but though no explanation can be offered it should be noted that no stone implement of any kind has been recorded from the Red Sea Province, although it is one of the best known parts of the Sudan, and has been the scene of considerable engineering efforts. This is the more remarkable in view of the geographical features of the country; the absence of forest, the weathered plateaux, the valleys filled with deposits through which innumerable wadis have been cut, all suggest that if stone implements existed some at least should have come to light. Much interest attaches to the distribu-

tion of ground stone axes in the Nile valley. While there is probably no museum with any pretence to an Egyptian collection which has not a number of these, and though they can be bought in almost every curio shop in Cairo, I have been unable to find any record of their discovery in a tomb group or undisturbed burial in Egypt; so that considering the number of prehistoric burials that have been examined, it can be said that they were scarcely if at all known in predynastic Egypt. On the other hand, they are common in Nubia, where a number have been found in predynastic and early dynastic tombs (**2** Pl. 63). Many examples have come from Meroe, and I believe that specimens occur on every site of neolithic date in the Sudan. They have certainly been found at Jebel Sabaat, at Jebel Geili 90 miles east of Khartum, at Jebel Gule and at Faragab. Moreover, the rock faces on which they were ground have been found both at Jebel Gule and Jebel Geili. We may therefore attribute a southern source to the ground stone axes of the Nile valley, and in the light of our present knowledge regard them as of Negro origin. This view is supported by the results of recent work on the prehistory of the Sahara. Gautier, who has devoted much time to the ethnography of the French Sudan, points out that while at the end of the neolithic period the northern Sahara had a stone industry characterised by unpolished implements of Egyptian affinities, in the central and southern Sahara the typical implement was the polished axe, and that this was of Sudanese Negro origin (**7**, 326). That the boundary of the two provinces seems to have coincided roughly with the present southern boundary of Algeria, *i.e.*, that the Berber-Negro frontier was then some 1,000 kilomètres further north than it is at present, is in no way opposed to this view. Perforated discs or rings and hollow conical and spherical stones, all ground in the usual neolithic style, have been found, notably at Meroe, on Jebel Haraza (W. Kordofan) and at Jebel Geili, where I believe stone discs and axe heads can be definitely associated together. Besides the types already alluded to Jebel Gule yielded a large number of pygmy implements of quartz, carnelian, and hornstone. These are similar to those found in South Africa and attributed to Bushmen, and there is reason to believe that this industry also existed at Faragab, where the innumerable disc beads of ostrich egg shell were probably bored with more or less worked up slivers of quartz.

The distribution of stone arrow heads in the Nile valley is also of much interest, but the data are as yet insufficient for discussion. It may, however, be worth while to point out that the transverse arrow head with its chisel-like edge cannot be of Negro origin, although the wooden figures of Negro bowmen from the tomb of Emseht, a general of the XI. dynasty, are armed with arrows with heads of this form, and the same type of arrow head is in use to this day among the tribes of the Kwilu and Kasai districts of the Belgian Congo.

Some mention must be made of the existence of stone monuments of megalithic type in the Sudan, although their number is small and their origin obscure. There is a monolith about two metres high on the plateau overlooking the Khor el Arab near the Sinkat-Erkowit road, to which tradition says Mohammed tied his horse. Another monolith of much the same dimensions has been described and figured by Crowfoot from Isa Derheib inland from Akik (**4** Pl.). At present there seems no reason to attribute any great antiquity to these stones, presumably they are connected with the upright stones and 'stelai' of Axum. Probably other rude stone monuments will be found in the Red Sea Province, indeed, I have heard of such, though the information was never very precise. It is, however, worth noting that typical dolmens do occur in the Madi country in the southern Sudan (**26**, 123).[1]

[1] When the time comes to estimate the significance of the Sudanese monuments of megalithic types, it will be necessary to remember that although uncommon, they do occur in Egypt. Indeed, Déchelette has suggested that their rarity in Egypt is only due to their having been nearly all destroyed. Meanwhile it may be noted that de Morgan has published the drawing of a stone circle at Jebel Genamieh in the desert to the east of Edfu; that N. de G. Davies found perfect miniature dolmens or cists at Tel el Amarna; that somewhat similar structures containing human remains have been described by Schweinfurth near El Kab; and that I have found them on the high desert plateau a few miles north of Abydos.

Although neither in structure nor form megalithic, I may here refer to a type of stone monument some 80 feet long and about 5 feet high occurring in some number in the neighbourhood of Erkowit in the Red Sea Province. The whole is constructed of stones without any cement or mortar; the face consists of a limiting wall of more or less flat slabs of local rock, while the spaces between the containing walls are filled in with smaller fragments. Each of these monuments may be considered as consisting of three main elements—viz. (i) an oblong rectangular portion, (ii) two oval masses to each of which is attached an expansion shaped somewhat like a fish's tail, and (iii) the curved walls uniting the other elements. Their orientation is definite within certain limits, while each oval element is interrupted at a constant point in its circumference, by the interposition of from two to four upright slabs. There is no chamber or space behind these stones, but from their constancy and the uniformity of their position it is obvious that they must have had a perfectly definite significance to the builders, and on account of their similarity to the false doors of Egyptian monuments I venture to call them 'false entrances.' The whole structure is quite unlike any others of which I have been able to hear, nor did the excavation of one much damaged example throw any light on the problem, but the false entrance suggests a funerary purpose and an Egyptian origin. In date they are probably mediæval, and may certainly be taken to antedate the spread of Mohammedan influence which was becoming dominant towards the middle of the fifteenth century (**20**).

The only rock pictures as yet found in the Sudan are in northern Kordofan. For the most part they are outlined in red or blackish pigment, but a few examples occur chipped on lumps of granite, on the hillside at Jebel Kurkayla in the Jebel Haraza *massif*. These figures are very rough, and the examples reproduced by H. A. MacMichael all represent camels (**11**). Drawings with pigmented outlines are found on Jebel Haraza and Jebel Afarit, and from the artistic standpoint seem to form two groups. To the first belong rough but spirited sketches of men on horseback, camels, and giraffes. The workmanship of the second group is rougher and much less vigorous; it includes representations of camels, men on horseback, and men marching or dancing carrying the small round Hamitic shield. This, together with their general resemblance to the 'Libyo-Berber' rock pictures of the southern Sahara, indicates a comparatively recent date for these drawings. Moreover, MacMichael notes that the work is faint and indeterminate, and that there is no trace of graving; in other words, the neolithic tradition has not persisted. Probably they are more recent than the stone discs and hollow conical and spherical stones found on Jebel Haraza to which I have already referred.

One of the most difficult questions arising in connection with the Sudan is that of Ancient Egyptian influence. Its existence may be readily granted, but what of its extent and duration? For while it is a platitude to say that a great and powerful state with a uniform tradition lasting for thousands of years cannot but have influenced the countries on every side, it must be confessed that where history fails the evidence is often extremely difficult to interpret. Every custom which at first sight seems to betoken Egyptian influence must be closely examined, and the evidence carefully sifted, to determine whether it may not have had its origin in the older and more generalised Hamitic culture of northern and eastern Africa. In discussing the value of the data upon which ideas and customs are to be traced back to an Egyptian origin, it is important to remember that general resemblances, either in widely distributed forms of social organisation and belief (*e.q.*, matrilineal descent, cult of the dead, &c.), or in widely diffused technical devices (*e.g.*, bow and arrow), cannot be admitted as good evidence. Whatever the future may bring, I do not think that in the present state of anthropological science even extreme and unusual beliefs and devices (which at first sight seem so strikingly convincing) should be considered as proof of common influence; otherwise it would be necessary to admit, immediately and without consideration, a cultural relationship between Papua and Central Brazil on the evidence of the phleme-bow, and between England and the Malay States on that of the fire-piston.[2] It is only when there is a considerable consensus of

[2] The distribution of the fire-piston in the East and its discovery as a 'scientific toy' in Europe has been discussed by Mr. Henry Balfour in *Anthropological Essays presented to Edward Burnett Taylor*.

agreement in underlying ideas and (or) in highly specialised customs or devices, that we are justified in considering an Egyptian origin, and even then it is necessary to bear in mind the possibilities of common ethnic origin and of 'convergence.' It is obvious that under these conditions facts will be differently interpreted, and opinions will vary within wide limits, while new discoveries may at any moment disturb views hitherto regarded as well founded.

Although in this address I propose generally to confine myself to the area included in the Anglo-Egyptian Sudan, yet in considering the question of Egyptian influence in Negro Africa I shall overstep these limits. The reasons for doing so are, I think, obvious. The data will not be abundant, and will not be restricted to the Anglo-Egyptian Sudan, while instances occurring outside that area will possess the same evidential value. Moreover, the records from the Belgian Congo, for example, are more numerous, while recent work in the north-west of Africa has provided material of much value from this comparatively new point of view. Thus, I shall not hesitate to cite West African instances, even though it is probable that the cultural drift responsible for them crossed North Africa and travelled down the west coast. It must not be forgotten that North Africa was permeated with Egyptian influence during the last few centuries B.C. Evidence for this statement might be drawn from many sources, but I will cite only one, that offered by the coinage, which is particularly convincing. In the third century B.C. many Carthaginian electrum and bronze coins bore the disc and uræi (**12** ii. 85, 93); a little later the coins of Numidia also bore Egyptian symbols, while in the first century B.C. those of Bogud II. of Mauritania (50-38 B.C.) were stamped with the winged disc. One of his successors Juba II. married an Egyptian lady Cleopatra Selene daughter of Mark Antony and Cleopatra, and, while his coins bear the uræus, those struck in her name bear the sistrum, the crown of Isis, and such Nilotic animals as the crocodile and hippopotamus (**12**, iii. 17, 95, 105, 110). These facts become the more significant when it is remembered that the coinage of the Ptolemies, the only pre-Roman coinage that Egypt had, was derived from the Greek and bore no Egyptian symbols.

Some will have it that we are faced with yet another difficulty; an Egyptologist has recently produced a work showing evidence of much patient research, in which he argues that a number of bloodthirsty rites, which he states occurred in predynastic and early dynastic Egypt, are closely related to those practised in West Africa at the present day, and indicate that the Egyptian religion is essentially 'African' in origin. His use of this word seems to indicate that he employs it as the equivalent of 'Negro,' though the latter word is never actually used. I believe that there is good and valid evidence against this view, both on the physical and the cultural side. All the work of the Archæological Survey of Nubia confirms the idea that the predynastic Egyptians were not even negroid, and it has extended this conclusion to the early Nubians who lived during the first three or four Egyptian dynasties. On the cultural side the evidence, though less absolute in that the data cannot be so objective as those supplied by long series of physical measurements, is none the less clear. It is generally held that the *sed* festival of Ancient Egypt was a survival, in a much weakened form, of the ceremonial killing of the king, and that its essential element was the identification of the king with Osiris. It was an important ceremony which persisted through the whole historic period, and was so much to the Pharaoh's taste that it was enacted no less than six times during the reign of Rameses II. The oldest known representation of the ceremony is that on the mace head of King Narmer (Menes). The king is seated in a shrine at the top of nine steps, dressed as Osiris, and holding the flail. On one side in front of the king are a number of standards, the first bearing the jackal Upwawet, 'the opener of the ways,' described on the seal of King Zer of the I. dynasty as 'he who opens the way when thou advancest towards the under-world.' The seal shows King Zer as Osiris preceded by Upwawet and 'the *shed-shed* which is in front,' the emblem of lightness or space. It is well known that kings are still killed ceremonially in Negro Africa at the present day; there are also rites which seem to be weakened forms of the same ceremony in which the king is permitted to survive. Assuming the above hypothesis of the origin of the *sed* festival to be correct, it cannot be seriously argued that the early Egyptians took over the custom from

the Negroes, at a time when Negro influence was so slight that no considerable amount of intermarriage had occurred. A custom which if it were not originally Egyptian would have introduced a totally new outlook on the world, and would have been intensely unpopular with the ruling powers.

With regard to the mode in which Egyptian influence was exerted on the Sudan there are three main routes along which we might expect to find its traces. The first is southwards along the Nile, the other two are to the west; one route at first following the Mediterranean coast but broadening westward as conditions become more favourable, the other running south-west through the oases and so communicating with Darfur and the Chad basin. Yet another route has been suggested by Sir Harry Johnston, namely, through Abyssinia and Somaliland, presumably reaching them *via* the Red Sea (**8**, 487). Perhaps it was by this route that the sistrum, still used in the church festivals, reached Abyssinia.

The extension of Egyptian rule up the Nile Valley can be traced from the earliest times to the XVIII. dynasty. But although after this Egyptian domination becomes less marked, Egyptian influence had become so firmly established that the culture of the states in the Nile Valley had a predominantly Egyptian tinge;[3] first Napata, then Meroe and then further south the states which we know later as the Christian kingdoms of Dongola and 'Alwah.

On *a priori* grounds the Nile route might be expected to be the most worn and the easiest to trace. For thousands of years Egyptian and Negro were in contact on the middle reaches of the great river, so that at least one great negroid kingdom arose; and though to this day a Negro dialect is spoken as far north as Aswan, yet at the present time there does not seem to be a single object or cultural characteristic which unequivocally can be said to have reached the zone of luxuriant tropical vegetation by way of the Nile Valley. The evidence for the earliest spread of Egyptian influence is set forth in the *Reports of the Archæological Survey of Nubia*, and, referring as it does to the country north of Wady Halfa, is outside the scope of this address. Yet I cannot leave it altogether unnoticed since it is intimately related to the discoveries recently made by Professor Reisner at Kerma in Dongola Province beyond the Third Cataract. (**17**.) In the Reports, Professor Elliot Smith shows that beyond Aswan, as far south as exploration has proceeded, the basis of the ancient population from the earliest times to the end of the Middle Empire was essentially of proto-Egyptian type, and that this type became progressively modified by dynastic Egyptian influence from the north, and Negro and Negroid influence from the south. As a result the Nubians contemporary with the New Empire present such pronounced Negroid characteristics as to form a group (the C-group) which stands apart from its Nubian and Egyptian predecessors. (**2**, Bulletin iii. 25.) The recent discoveries at Kerma, which include fragments of alabaster jars with the names of kings of the VI. and XII. dynasties and also seal impressions of the Hyksos period, have shown that here was a fort or trading post certainly occupied during the Hyksos period and probably as far back as the VI. dynasty. It is the remains of the Hyksos period that are especially interesting. Professor Reisner describes a people who razed the buildings of their predecessors, and buried their dead in the débris, who battered the statues of Egyptian kings of the XII. dynasty, and whose funerary customs were entirely un-Egyptian. Each burial pit contains a number of graves in every one of which several bodies had been interred. The chief personage lies on a carved bed; 'under his head is a wooden pillow; between his legs a sword or dagger; beside his feet cowhide sandals and an ostrich-feather fan. At his feet is buried a ram, often with ivory knobs on the tips of the horns to prevent goring. Around the bed lie a varying number of bodies, male and female, all contracted on the right side,

[3] The early Ethiopian kings used the Egyptian language and writing for their records; it was only towards the end of the Meroitic period after the downfall of Egypt, that the Meroitic language was written. A special hieroglyphic alphabet founded on the Egyptian may date back to the third century B.C., but the actual Meroitic script is later than this; Crowfoot indeed argues for so late and short a range as from the middle of the second to the fourth century A.D. (Griffith, *The Meroitic Inscriptions of Shablûl and Karanog*, chap. ii.).

head east. Among them are the pots and pans, the cosmetic jars, the stools, and other objects. Over the whole burial is spread a great ox-hide. . . . Several had their fingers twisted into their hair or had covered their faces with their hands. One woman had struggled over on her back and was clutching her throat. But most of them lay composed as if minded to die quietly, according to the custom of their fathers.' (**17**, xii. 24.) Reisner could not observe any marks of violence, but, judging from the contorted positions of some of the bodies, thought that they had been buried alive. The remains from these burials have been examined by Elliot Smith, who states that the skeletons surrounding the bedstead are those of folk of proto-Egyptian and Middle Nubian (C-group) types, while those on the beds belonged to typical New Empire Egyptians, such as lived in the Thebaid at this time. (**23**, 228.)

The first historical capital of the Sudan was Napata, the mediæval Merowe or Merawi, near Jebel Barkal, between the 19th and 20th parallels of latitude, a few miles south of the Fourth Cataract. Napata was certainly an important place in the XVIII. dynasty but how much earlier is uncertain. In the XX. dynasty the high priest of Ammon assumed the viceregency of Nubia, and even Rameses XII., the last and feeblest king of this dynasty, retained Thebes and Nubia though the delta had become independent. Remembering the troubles and revolts of the XXI. and XXII. dynasties and the expulsion of the nobles of Thebes we may believe that the priestly families of Thebes were forced to flee to the more remote parts of Nubia, and so set up at Napata a kingdom which, in theory at least, reproduced the theocracy of Ammon at Thebes. The first recorded lord of this new kingdom was Kashta, whose son Piankhi succeeded him about 741 B.C. and by 721 B.C. had conquered and garrisoned Egypt almost as far north as the Fayum. Later he received the submission of the Lords of the Delta, and was succeeded by his brother Shabaka, who ruled all Egypt and founded the XXV. (Nubian) dynasty which lasted at least fifty years. Thus Napata was Egyptianised, and being a great trading centre cannot but have influenced profoundly the country to the south, so that when Meroe was founded in the eighth century B.C. the ruling influence must have been Egyptian. The mission sent by Nero to explore the Nile reported that Meroe was ruled by a Queen Candace, whose predecessors had borne that name for many generations. (**16**. Bk. vi. chap. 35.) It is uncertain whether the Candace of Nero's time was one with the Candace (Kantakit) who was buried in a Meroe pyramid, or whether the latter was identical with the queen who attacked Egypt during the reign of Augustus. However the matter may stand, we know of two if not three queens bearing the same name. Yet, since the monuments show that a king was generally the head of the state, Pliny's assertion requires qualification; moreover, there is the perfectly definite reference to King Ergamenes slaughtering the priests who, as was the custom, had determined his death. In both statements I cannot but see examples of Egyptian theocratic influence. Nor are they mutually destructive if it be remembered that the throne might, and often did, pass in the female line, and that this practice was known to be in full force during the XVIII. and later dynasties.[4]

It would be entirely consonant with the policy of the priests of Ammon to take advantage of the spirit of the *sed* festival, the rite of ceremonial Osirification practised by the Egyptian kings, in order to obtain for themselves absolute political concord. This would be the easier if among the barbaric tribes in southern Nubia the king was ceremonially killed as he recently was in Fazogli, and as he still is among the Nilotes. That the practice of slaying the king was no new priestly device is, I think, clear from the account given by Strabo, and though I have no space to analyse this, I may point out that it accords well with our knowledge of the divine kings existing in the Nile valley at the present day, or in recent times. Strabo's description makes clear how relatively narrow

[4] Paynozem I. of the XXI. dynasty married the daughter of the Tanite king, Pesibkhenno I. and Sheshonk the founder of the XXI. dynasty legiti mised his line by marrying his son to the daughter of Pesibkhenno II., the last king of the preceding dynasty. So, too, in order to hold the treasure of Ammon at Thebes, Piankhi caused Shepnupet the daughter of Osorkon III., sacerdotal priestess of Thebes, to adopt his sister-wife Amenardis.

was the stream of northern civilisation which penetrated Black Africa by way of the Nile valley. But even this civilisation did not come with a steady flow; when Egypt prospered under the early Ptolemies Meroe prospered; as Egypt decayed Meroe fell into the wretched condition recorded by Nero's officers; and even before this Candace could assert that neither the name nor condition of Cæsar was known to her. (**24**, Bk. xvii. chap. 1.) As northern influence lessened, and the power of Meroe decayed, the black element would preponderate more and more, so that the travellers who visited Ethiopia told a story of barbarism and utter stagnation. The mode of life of the Ethiopians was wretched, they were for the most part naked wanderers, moving from place to place with their flocks. Even in the cities along the river banks the houses were as often made of wattle and wood as of bricks. Although they had grain from which they prepared beer, they had no fruits or oil, using butter and fat instead, and lived upon roots and the flesh and blood of animals, milk and cheese. (**24**, Bk. xvii. chap. 2.) Beyond the immediate neighbourhood of the river was a region of savagery, with inhabitants for the most part known by mere nicknames, root-eaters, fish-eaters and so forth. But even in the earlier and better days of Meroe there is evidence that suggests the presence of much black blood in the royal family. In a stele from Napata, now in the Louvre, the mother, wife and sister of King Aspelut (about 825 B.C.) are represented as distinctly steatopygous, while the representations of Queen Amen-Tarit at Meroe some three centuries later are frankly Negroid. Moreover, even while a king exerted real authority at Meroe it would be entirely consonant with African politics and African customs for vassal 'kingdoms' to arise at the extremes of the state. So, when it is recorded on the authority of Eratosthenes, that in the third century B.C. the Sembritæ who occupied an island south of Meroe were ruled by a 'queen' but recognised the suzerainty of Meroe (**24**, Bk. xvii. chap. 1), we may think of the petty chieftains of the eighteenth century who were the true rulers of the country from Dongola to Sennar, though every sultan of Sennar claimed sovereign rights. There may have been many such 'states' ruled by women, just as at the present day in the Nuba hills the highest authority passes in the female line, and may be exerted by a woman.

Meroe seems to have been destroyed before the introduction of Christianity. Nevertheless, two if not three culture phases can be traced in its history. There was first a period of Egyptian influence under King Aspelut and his successors, then came an influx of Greek ideas, a phase which Professor Garstang would date from about the third century B.C. This is the period to which most of the monuments now visible belong, and it was succeeded by the period of Roman dominance. At Soba, on the Blue Nile a few miles above Khartum, Lepsius collected the cartouches of a number of kings and queens of Meroe; this site, the capital of the Christian kingdom of 'Alwah, was certainly inhabited through mediæval times, and may not have been fully deserted till three or four hundred years ago. No doubt northern influence diminished progressively, especially after the conquest of Egypt by the Arabs, when the rivalry between Christianity and Islam which culminated in the Crusades must have had the effect of closing Upper Nubia and the countries beyond to Mediterranean influence. Nevertheless, trade objects continued to come through, though there is nothing to show whether these travelled up the Nile or across the Eastern Desert from the thriving Mohammedan trading ports which, as Crowfoot has shown, were established there previous to the thirteenth century.[5] (**4**, **542**.)

[5] There is a particular form of bead made of chalcedonic agate, numerous examples of which have been dug up in the neighbourhood of Khartum. A large number of similar beads are said to have been found in Somersetshire a few years ago, but archæologists tell me that there is no sufficient warrant for their origin. But a chain of these beads found at Erding in Upper Bavaria and now in the Munich Museum (K. IV. 1924 in Room 2, Case 4) dates them, at least approximately, to the early mediæval period, Merovingian or earlier. Moreover, three large chevron beads of the usual type were said to have been removed from the body of an 'Arab' who fell at Omdurman, and I see no reason to doubt the truth of the statement. What I have said concerning the Ethiopia of Strabo closely follows the argument set forth by Crowfoot in *The Island of Meroe*, published by the Egypt Exploration Fund.

No doubt the territory over which the rulers of 'Alwah exerted authority extended south of their capital, yet beyond Soba, in the archæologically unexplored country south of the confluence of the two rivers, traces of northern influence quickly become fewer and less distinct. Nevertheless, at the present day among the hills between the White and Blue Niles the name Soba is still known, being recognised as that of a series of great queens who ruled over a mighty empire of the same name. I cannot say how wide the area may be over which this belief is held; my information was obtained at Jebel Gule fifty miles east of Renk. At the foot of the hills are two settlements of a people who call themselves Fung, but who are generally known as Hameg. These people say that the great Queen Soba whom they worship was their ancestress, but they also apply her name to certain stones which they regard as sacred. The most important of these is a spherical water-worn stone (about 18 inches in diameter) of a brownish colour, with large quartz veins traversing it in every direction. This stone was stated to have been the 'throne' of Queen Sheba, and is still the 'chair of kingdom' (*kursi memlaka*) upon which every Hameg paramount chief (*mangil*) assumes office. Besides this rock there are two others associated with Soba, and hence called Soba. Both are weathered boulders, partly embedded in the soil at the side of the track round the base of Jebel Gule. A prayer given me by a woman at one of these rocks ran somewhat as follows: 'Grandmother Soba . . . permit us to go on our journey and return in safety.' There was obviously the utmost confusion in this woman's mind between Soba the goddess and Soba the stone on which she had just placed a handful of sand. Soba may also be asked to relieve sickness, and is invoked during a dance held by the neighbours of a recently delivered woman, about the time when the young mother is allowed to leave her house for the first time. Few will doubt that in the Soba of the Hameg belief there is preserved the memory of such queens as Candace the ruler of the Sembritæ, grafted on the recollection of the great city, which to the Negroids of the Gezira no doubt appeared to dominate the north. Nor do these traces of ancient tradition stand alone; at Jebel Moya near the Blue Nile some 150 miles south of Khartum there is actual archæological evidence of northern influence. Here besides stone implements were found beads, and amulets, a number of scarabs, and small plaques bearing Ethiopian and Egyptian cartouches ranging from about 700 B.C. (**25**, 617), or perhaps going back to an even earlier date. I may also note that on the as yet unexplored site of Faragab in northern Kordofan, besides potsherds, stone implements and ivory objects I have found a carnelian bead identified by Professor Petrie as of XVIII. dynasty make, as well as dolomite and scolecite beads which are certainly not of Negro workmanship or character.

Faragab must have been an important site for a considerable period, and would well repay systematic examination; the mounds, which are some ten feet high, occupy a considerable area about a mile N.W. of the modern village. The potsherds found in them are of special interest, and among them are the remains of a type of pot which, as far as I can discover, is different from any in use in Africa at the present day. This type was oblong and rather shallow, decorated with geometrical patterns and produced at each end into a solid mass more or less covered with designs; its shape, in fact, was that of many Melanesian food bowls. Another interesting feature of the Faragab sherds is that many of the larger vessels were made on string mats (**21**). The large narrow-necked vessels found in such numbers in the necropolis at Meroe were made by this method, which is still in use for water vessels in northern Kordofan.

These sites seem to mark the southern limit of Egyptian influence as far as the actual transmission of objects derived from the north is concerned. Of the racial affinities of the inhabitants of Faragab nothing is known, but we are better informed concerning the old residents of Jebel Moya. The cemeteries of this site have yielded the remains of a tall coarsely built Negro or Negroid race with extraordinarily massive skulls and jaws (**5**). In a general way they appear to resemble the coarser type of Nuba living in South Kordofan at the present day, and it is significant that the cranial indices of the men of Jebel Moya and the Nuba hills agree closely. Thus there is the clearest

evidence that Egyptian influence reached south of Khartum; and since it has persisted to the present day in oral tradition among the tribes of the little known country between the Blue and White Niles, traces might equally be expected among the Nilotes of the White Nile. But strangely enough nothing of the sort has been found, although the Shilluk and Dinka are better known than any other of the Sudan tribes. On the other hand, the tribes of the Congo basin have a number of customs which do suggest Egyptian influence, and the same may be said perhaps of Uganda, so that it seems reasonable to believe that Egyptian influence spread up the White Nile and passed westwards across the Nile-Congo watershed. An alternate route would be along the Blue Nile and its tributaries the Dinder and the Rahad to the Abyssinian hills, southward through the highlands to about 5° N., and thence westward to the head waters of the Congo.

To return to the Shilluk and Dinka, the most northern of the Negro tribes of the White Nile. The fact that no cultural elements which can be connected with Egypt are found on the White Nile, where they might have been expected, suggests, either that the tribes now occupying the district were not there when Egyptian influence spread south, or that the country presented such difficulties that the foreign stream left it on one side, as would have been the case had it followed the route *via* the Blue Nile and the highlands of Abyssinia. In other words, either the Shilluk and Dinka reached their present territory in comparatively recent times, or else led a wandering and precarious life in swamps as formidable as the Sudd of the present day. There is, I think, a good deal in favour of the latter view. The existence in the depths of the Sudd of Nuer communities, of which we know little except through rumour, shows that such a life is possible; while among the Dinka the Moin Tain or 'marshmen,' who possess no cattle and scarcely cultivate, but live by hunting and fishing, exist under almost as unfavourable conditions. Moreover, there is abundant evidence that North-West Africa is drier now than it was a few thousand years ago, and if those authors are right who state that there was a general melting of glaciers in Europe some 5000 years B.C., giving rise to widespread floods (the origin of the Biblical deluge), the increased precipitation may well have given rise to a considerable northern extension of the Nile swamps. In support of this argument it may be noted, that in numerous XVIII. dynasty paintings Negroes are represented with bows and arrows and throwing sticks (boomerangs), *i.e.*, their weapons are not those of the northern Negroes of the present day, the Shilluk and Dinka, who are not bowmen and do not use the throwing stick. Shilluk traditions state that they came from the south, and a language substantially identical with theirs is spoken by the Acholi of the Uganda Protectorate.

Evidence pointing in the same direction exists on the physical side; the results of the archæological survey of Nubia show that even in late dynastic times the tall Negroids (E-group) whose skeletons have been found near Shellal were mesaticephals (2, 80), with a cephalic index higher by three or four units than those of the Dinka and Shilluk respectively. On the other hand, a people with a cephalic index nearer that of the northern Nilotes had reached Nubia by the Byzantine-Pagan period (200-600 A.D.). Elliot Smith and Derry speak of these people (the X-group) as prognathous and flat-nosed Negroids who suddenly made their way north into Nubia (2, Bulletin V. 12). Sixteen X-group skulls (eleven male and five female) in the College of Surgeons give a cephalic index of 70·8, and comparing them with the series of about the same number of Dinka skulls in the collection, my impression is that as a group they show as many Negroid characters.

It seems reasonable to believe that the Negroids, whose skeletons form the majority of those found in Negro graves in Nubia, belonged to the group of Negroes with whom the Egyptians were in contact on their southern frontier. In other words, the tribes resembling the modern Dinka were so little in effective contact with the Egyptians, that they did not reach Nubia till some 1500 years ago, the cattle-owning Negroes whose representations were so frequent being the tall mesaticephals whose remains occur in Nubia in late dynastic times.

The numerous records of Negro incursions from the Middle Kingdom

onwards suggest that the Negroes were driven north in a succession of waves by some force from which this direction offered the only chance of escape. Such can only have been applied by other Negroes behind them. It may well be that there was more or less continual ferment on the southern border of Egypt in the early part of the first millennium B.C., and that the northern Nilotes were beginning to make their reputation as fighting men. Indeed, the passage in Isaiah can scarcely bear any other meaning than that this people was working north with sufficient energy for their peculiarities and those of their land to have become known to the Mediterranean world. 'Ah, the land of the rustling of wings, which is beyond the rivers of Ethiopia: that sendeth ambassadors by the sea, even in vessels of papyrus upon the waters, saying, Go, ye swift messengers, to a nation tall and smooth, to a people terrible from their beginning onward; a nation that meteth out and treadeth down, whose land the rivers divide!' (Isa. xviii. 1, 2, Revised Version). But while the tall Negroes seem to have been the first to reach Nubia in organised groups, stray examples of short brachycephalic Negroes (usually female) have been found as far back as protodynastic times. I am indebted to Professor Elliot Smith for the information that the four Negresses found in cemetery No. 79 at Gerf Hussein (2, Bull. iii. 22) were short in stature with relatively broad oval crania, while at Dabod in a Middle-Kingdom cemetery there was found a skeleton of a man measuring 1·61 m. (about 5 feet 3 inches), with definite prognathism, typical Negro hair, and a cephalic index of 80 (2, ii. 121). Presumably these were representatives of the group of short mesaticephalic Negroes who are at the present time found on both sides of the Nile-Congo divide, but predominantly west of it, a group represented by the Bongo, Azande, and cognate tribes. We thus reach the position that the Nubians, who were proto-Egyptians, were, in the earlier part of their history, in contact with just that class of Negroes among whom customs and ideas apparently of Egyptian origin are found at the present day. It must not, however, be assumed that it was this contact that led to the dissemination of Egyptian ideas, indeed our present information suggests that it can scarcely have been sufficiently intense.

The following table, giving the measurements and indices available for the comparison of the E-group Negroids with the tall Negroes of the present day, shows that the former belonged to the mesaticephalic group, which includes the Burun, the Bari, and the Nuba. As regards head length, head breadth, cephalic index and stature, the E-group stands closer to the Nuba than to the other tribes, while even in head breadth it is as near the Nuba as the Dinka.

——	H.L.	H.B.	C.I.	Stature
Shilluk	195	139	71·3	1,776
Dinka	194	141	72·7	1,786
E-group	190 [6]	143 [6]	75·68 [6]	1,723
Nuba	190	145	76·6	1,722
Burun	190	150	79·16	1,759
Bari	190	149	78	1,741

At the present day the mesaticephalic group includes the Hameg and the Berta of the hills between the White and Blue Niles. The excavations at Jebel Moya—also between these two rivers—have enabled Dr. Derry to show that in Ptolemaic times this hill stronghold was inhabited by tall mesaticephali with a cephalic index almost identical with that of the Nuba, so that we are led to conclude that all these tribes, including the E-group Negroids, belong to one and the same stock.

A number of similarities between Ancient Egypt and Modern Africa have been set out recently by Professor Petrie (15). He does not discuss the routes by which Egyptian influence may have reached Negroland, but simply marshals the evidence of similarity under sixty-one headings. A good many of these are

[6] The H.L. and H.B. of the E-group skulls have been increased by 7 mm. and 8·5 mm. respectively in order to make these measurements comparable with those on the living. For the same reason the C.I. has been increased by 2 units.

so widely spread outside Africa as to be of little evidential value; others, and this applies specially to material products, include such simple or obvious devices that they can scarcely be regarded as carrying weight; but there are a number of instances which are highly suggestive, and when to these are added yet other habits and customs common to Ancient Egypt and Negro Africa, a mass of evidence is presented which seems decisively indicative of Egyptian influence. This view does not imply that all the features common to Ancient Egypt and present-day Negroes are instances of borrowing; on the contrary, I hold that many common customs are but expressions of the wide diffusion of old Hamitic blood and ideas. To this ancient stratum I would attribute those customs which I have discussed in a previous paper (22), including burial by the Nilotes in the crouched position, the use of the throwing-stick (boomerang) by the Beja, and the killing of the divine king (or rainmaker).

The ideas and customs reported from tropical Africa which may be due to Egyptian influence may be classified provisionally in the following groups:

(i) Beliefs connected with the soul.
(ii) Beliefs and customs connected with the king or the royal office.
(iii) Death customs.

(i) Beliefs concerning the soul. The Egyptians believed that each individual possessed several souls, the most important being (*a*) the *ka* or human double; (*b*) the *ba*, the bird soul which accompanied Ra in the sun barque through the other-world; (*c*) the *akh* or *khu* (fate unknown); (*d*) the *khaibet* or shadow; and (*e*) the *ab* or heart; that in one aspect at least this was something more than the physical heart is shown by the fact that after death the *ab* was weighed by Anubis in the presence of Osiris against the feather of truth. Numerous instances of 'multiple souls' resembling those of Egypt have been recorded from the Congo and West Africa; perhaps the most striking is afforded by the Bambala, who say that man is composed of four parts, the body *lo*, the double *ilo*, the soul *n'shanga*, and the shadow *lumelume*, while some have an additional element called *moena* (**1**, vol. ii. fasc. i. 124).

(ii) Beliefs and customs connected with the king or the royal office. The fact that the Pharaoh, the Shilluk king, the Dinka rainmaker, and many other African rulers were, or are, divine kings, who were at one time ceremonially killed and with whose bodily well-being the welfare of their kingdom was bound up, does not, to my mind, indicate borrowing, but points rather to a common origin, for I believe that the divine king is an old Hamitic institution, and that Hamitic blood or cultural influence has been active over a very great part of Negro Africa. On the other hand, there does seem a possible connection between the Egyptian Horus and the eagle totem of the Baganda king. In Egypt, once totemistic, the king was identified with the falcon, and was spoken of as Horus; the falcon was placed over one of his names, and on the great slate palette of Narmer, dating back to protodynastic times, the falcon (with one human arm) leading captive the north country obviously represents the king. Among the Baganda every king and prince, in addition to his clan totem, claimed the eagle totem, though no eagle clan existed [7] (**18**, **128**).

(iii) Death customs. The majority of the customs which can probably be attributed to Egyptian influence are associated with death and burial; this is not surprising, considering how greatly the Egyptians were concerned with the after-life. Let us consider the main features of Egyptian burial in the following order: posture of the body in the grave, preparation of the body and coffin, form of the grave, offerings in and at the grave, worship at the grave. I have shown elsewhere that the custom of burying on the side in the embryonic position was widespread in Africa among peoples of Hamitic blood; but I do not regard this as due to Egyptian influence, but rather as part of a common Hamitic heritage.

In Egypt the body was prepared for the grave by an elaborate process of mummification; it was then enclosed in a coffin often of anthropoid shape. In tropical Africa numerous instances of attempts to preserve the body are recorded. In Uganda the body of the king was opened, the bowels removed, emptied,

[7] Dr. Allan Gardiner informs me that even in Egypt there are indications that the eagle and falcon were confused in Hellenistic times.

washed in beer, dried, and then replaced (**18**, 105), while among the Banyoro (**3**, 316) and the Makaraka (**9**, i. 297, ii. 361) other methods were adopted. It seems a far cry from the mummies of Egypt to the smoke-dried corpses of Equatoria, and it is not difficult to see that ancestor worship might easily give rise to attempts to preserve the body when everyday experience would suggest desiccation or smoking, but there are certain Congo tribes whose practices do suggest an actual link with Egypt. Among the Wambunda of Stanley Pool the body is placed in the squatting posture, the limbs are tightly flexed on the body and tied in that position, the whole being packed with a large quantity of spongy moss which is kept *in situ* by bandages. A gentle fire is kept up round the body for two or three months, after which it is rolled in native cloths and buried (**1**, fasc. ii. 176). The latter part of this ceremony hints that the attempts to preserve the corpse may have been imposed on an older habit of speedy burial; such an imposition could only have come from without.

Among the Wangata an important person of either sex is buried in a massive coffin with a lid carved to represent the deceased (**1**, i. fasc. ii. 180). It is difficult not to believe that here is an echo of the Egyptian mummy case. If this be so, may not the practice of a tribe near Lake Leopold II., who after a rough preparation of the body roll it in native cloth and place it in a canoe-shaped coffin (**1**, fasc. ii. 175), be regarded as connected with the funerary boats of Egyptian burial ceremony? Since the anthropoid coffin was unknown before the XI. dynasty, it follows that the northern influence must have been exerted after this period. Egypt's first great expansion (after the Pyramid builders) dates from the XII. dynasty, when Egyptian and Negro were in intimate contact at the Second Cataract, as shown by the celebrated decree of Senusert III. Further, about this time special importance seems to have been attributed to the funerary voyage on the Nile, indeed almost all the models of funerary boats in our collections are of this period.

If these facts be accepted as evidence of the date at which Egyptian ideas influenced Equatorial Africa, there are other customs which seem to indicate that this was not the only period of such cultural drift. The coffins of the III. and IV. dynasties were often large rectangular boxes designed and painted to represent houses. Now the Mayumbe roll the body of a dead chief in layers of cloth and place it in an enormous wooden coffin of rectangular shape, the top of which is carved to present a homestead (**1**, i. fasc. ii. 177). Again, the funeral ceremonial of the Ndolo seems reminiscent of this period. Immediately after death the Ndolo prepare the body, painting it red, touching up the eyebrows with charcoal, and propping it up with open eyes and mouth on a high seat in the very posture of the *ka* statues of the Pyramid-builders, *i.e.* seated with the forearms and hands upon the thighs, a position which I venture to say no Negro would adopt. The body remains here for a day, while more or less continual drumming and dancing goes on, and is then buried (**1**, i. fasc. ii. 176).

If I have not laid too much stress on the XII. dynasty *liaison*, it would seem that the cultural drift originating the Ndolo custom was earlier than that affecting the Wangata and the Mayumbe. Professor Elliot Smith tells me that in Egypt bodies were not deliberately painted red before the XXI. dynasty, but it is possible that the Ndolo confused the dead body with the effigy and—for the time of its exposure—treated it as the Egyptians treated their wooden statues from an early date onwards. Nor must it be forgotten that at the present day the Bari paint themselves red.

An Egyptian mastaba tomb consists of (i) a tomb chapel, which may or may not lead to a series of chambers. There may or may not be a *serdab*. The tomb chapel is distinguished by the false door which is placed on the west wall and faces east. Behind the false door is (ii) a pit. The burial may be at the bottom of the pit or (iii) in a recess at the side. The recess may lengthen out into a passage with (iv) a chamber at the end. The form of the tomb was to a very great extent the expression of the Egyptian belief that the soul, or souls, of the deceased visited the body in the tomb chamber, coming in and out by the shaft of the pit, and indeed the XVIII. dynasty papyrus of the priest Nebqed represents the human-headed *ba*-soul descending the shaft to visit the mummy (**13**, Pl. iv.). These beliefs also led to the burial of supernumerary stone heads to which the soul might attach itself should the body perish.

Recently eight life-size portrait heads of a princess and the courtiers of the court of Chephren have been found in the mastabas at Gizeh constituting the royal cemetery of the IV. dynasty (**17**, xiii.). The cartonnage busts, presumably of the deceased, represented as carried in funeral processions of the Middle Empire (*cf. e.g.*, Stele C 15 in the Louvre), are probably a development of the same idea. Similar expressions of belief occur in Negro Africa, the examples being too numerous and the resemblances too exact for this to be due to any other cause than actual borrowing.

I am indebted to Messrs. Torday and Joyce for the information that the Tofoke in the neighbourhood of the Stanley Falls make a grave which in principle and form comes extremely close to the ancient Egyptian. A circular pit is dug, about 6 feet in diameter and 8 feet in depth; about 2 feet from the floor is a horizontal passage, about 15 feet in length, and of sufficient size to allow the body to be pushed along it; at the end of the passage is a vertical shaft, not more than 1 foot in diameter, reaching to the surface of the ground. The grave is lined with mats, and the body is pushed into the horizontal passage; the main shaft is filled in, but the smaller shaft, being destined for the passage of the soul, is merely covered with branches and a little earth. Over the grave a conical mound is erected, and on this a shed; here are put food and certain property of the deceased after the mound has been covered with charcoal. A similar tomb, but without the special shaft for the soul and hence even more closely resembling the Egyptian mastaba grave, has been figured by Froebenius (**6**, Pl. i.). The body, apparently tightly covered with wrappings and with vessels for offerings near it, lay in a lateral chamber at the bottom of the pit. The mouth of the pit was covered by a mud dome, on the top of which there was a pot, the whole having a hut built over it. A small aperture on the western side of the dome gave access to the shaft.

In the Pyramids the parts of the mastaba tomb underwent special treatment; in the Meydum pyramid a sloping passage takes the place of the shaft, the passage probably being reminiscent of the sloping stairway of the proto-dynastic mastaba. The so-called 'trial passage' near the Great Pyramid, as well as the passages within it, seem reminiscent of a tomb with two sloping passages running in the same vertical plane and meeting at an obtuse angle. This type, which may be called the double sloping passage tomb, is also found in Negro Africa, namely in Lower Senegal and Northern Hausaland, between 13° and 18° N. 'First . . . passages were dug under the earth and, at their coincidence, the gallery was enlarged, as the first sketch of a building with an oval-shaped dome. This dome was panelled and strengthened with wood from the Borassus palm. This domed underground vault contained the dead man and a good many things besides. . . . The Eastern hole was filled in, but the Western one was sealed with boards and only opened yearly to receive fresh offerings. A second and very strong dome, to which a covered passage gave access from the west, was raised on the surface exactly over the roof of the grave-chamber proper . . . and the mound was piled high over the whole. . . . The entrance to the grave itself, which was opened but once a year for the insertion of the autumnal offering, was covered with planks laid horizontally. But on all other occasions the priests held intercourse with the dead in the upper chamber' (**6**, i. 25, 26).

Offerings made at the grave, and worship performed there, are so inextricably interwoven that they may be considered together. It is probably true to say that every Negro tribe has ancestor worship in some form or another, and at some time makes offerings at the ancestral graves; but the following instance from New Calabar is so suggestive of ancient Egyptian influence, and mimics the supernumerary heads and cartonnage busts (already cited) so closely, that it does not seem necessary to give any other example. The *duen-fubara* is a carved and painted image representing the head and shoulders of the deceased. This is brought by night in solemn procession to the dead man's village a year after his death, and deposited for eight days in charge of someone of importance in the community. On the eighth day goats and fowls are sacrificed by the eldest son of the deceased before the *duen-fubara*, which is sprinkled with the blood. After a mimic battle in which the sons of the deceased are opposed by the men of the house in which the image was deposited, the latter yield, and the *duen-fubara* is carried in solemn procession

to the place prepared for it. Here it is placed 'in the hall or outer room of a house which has been specially built for the purpose and . . . the . . . house chapel—now consecrated by the spiritual presence, which has been previously invoked and conjured into this special emblem—is daily swept and cleaned' (**10**, 162-164).

To sum up: concerning the early prehistory of the Anglo-Egyptian Sudan we have no more than indications. In the Neolithic stage, which appears to have persisted until a comparatively recent date, Negro influence, if not predominant over the whole area, was at least powerfully felt even in the north, as is shown by the distribution of polished axe-heads. But against this northward pressure must be set the continual extension of Egyptian culture, the evidence for which may best be found in the eschatological ideas and burial customs ('mummification' and anthropoid coffins) of the peoples of Equatoria. This influence, which seems to have persisted until mediæval times, may have reached tropical Negroland as early as the Middle or even the Old Kingdom. Nor was the Nile route the only one by which Egyptian influence was spread. Another and later drift extended westwards as shown by the coinage of the north African States, which enables us to fix its date within fairly precise limits. We do not know how far south this drift travelled, but it seems certain that it reached at least as far as the Senegal River and the great bend of the Niger.

Bibliography.

1. *Annales du Musée du Congo*, Series III. (Brussels).

2. *Archæological Survey of Nubia* (Cairo).

3. Baker, Sir Samuel, *Ismailia*, 1879.

4. Crowfoot, J. W., 'Some Red Sea Ports in the Anglo-Egyptian Sudan,' *Geographical Journal*, 1911.

5. Derry, D. E. In a paper read before the Anthropological Institute, as yet unpublished.

6. Froebenius, Leo, *Voice of Africa*, 1913.

7. Gautier, E. F., 'Etudes d'Ethnographie Saharienne,' *L'Anthropologie*, 1907.

8. Johnston, Sir Harry, *Uganda Protectorate*.

9. Junker, Wilhelm, *Travels in Africa*, 1890.

10. Leonard, A. G., *The Lower Niger and its Tributaries*.

11. MacMichael, H. A., 'Rock Pictures in North Kordofan,' *Journ. Roy. Anthrop. Inst.*, 1909.

12. Müller, L., *Numismatique de l'ancienne Afrique, Copenhagen*, 1861.

13. Naville, E., *Das Aegyptische Todtenbuch* (Berlin), 1886.

14. Peringuey, L., 'The Stone Ages of South Africa,' *Annals of the South African Museum*, Vol. viii.

15. Petrie, W. M. Flinders, 'Egypt in Africa,' *Ancient Egypt*, 1914.

16. Pliny, *Natural History*.

17. Reisner, G. A., *Museum of Fine Arts Bulletin* (Boston).

18. Roscoe, John, *The Baganda*.

19. Seligman, C. G., 'A Neolithic Site in the Anglo-Egyptian Sudan,' *Journ. Roy. Anthrop. Inst.*, 1910.

20. Seligman, C. G., 'An Undescribed type of building in the Eastern Province of the Anglo-Egyptian Sudan,' *Journal of Egyptian Archæology*, 1915.

21. Seligman, C. G., 'A Prehistoric Site in Northern Kordofan,' *Annals of Archæology and Anthropology* (Liverpool), 1915.

22. Seligman, C. G., 'Some Aspects of the Hamitic Problem in the Anglo-Egyptian Sudan,' *Journ. Roy. Anthrop. Inst.*, 1913.

23. Smith, G. Elliot, *Report of the British Association*, 1914.

24. Strabo, *Geography*.

25. Wellcome, Henry S., *Report of the British Association*, 1912.

26. Wilson and Felkin, *Uganda and the Egyptian Sudan*.

British Association for the Advancement of Science.

SECTION I: MANCHESTER, 1915.

ADDRESS

TO THE

PHYSIOLOGICAL SECTION

BY

PROFESSOR W. M. BAYLISS, M.A., D.Sc., F.R.S.,

PRESIDENT OF THE SECTION.

The Physiological Importance of Phase Boundaries.

EVEN a hasty consideration of the arrangements present in living cells is sufficient to bring conviction that the physical and chemical systems concerned operate under conditions very different from those of reactions taking place between substances in true solution. We become aware of the fact that there are numerous constituents of the cell which do not mix with one another. In other words, the cell system is one of many 'phases,' to use the expression introduced by Willard Gibbs.

Further, parts of this system which appear homogeneous under the ordinary microscope are shown by the ultra-microscope to be themselves heterogeneous. These are in what is known as the colloidal state. Some dispute has taken place as to whether this state is properly to be called a heterogeneous one, but it is sufficient for our purpose to note that investigation shows that the interfaces of contact between the components of such systems are the seat of the various forms of energy which we meet with in the case of systems obviously consisting of phases which can be separated mechanically, so that considerations applying to coarsely heterogeneous systems apply also to colloidal systems. Although the phases of a colloidal system cannot be so obviously and easily separated as those of an ordinary heterogeneous one, this can be done almost completely by filtration through membranes such as the gelatin in Martin's process. To avoid confusion, however, it has been suggested that the colloidal state should be spoken of as 'micro-heterogeneous.' There are, in fact, certain phenomena more or less peculiar to the colloidal state and due to the influence of the sharp curvature of the surfaces of the minutely subdivided phase. The effect of this curvature is a considerable pressure in the interior of the phase, owing to the surface tension, and it adds further complexity to the properties manifested by it.

We see, then, that the chemical reactions of chief importance to us as physiologists are those known as 'heterogeneous.' This class of reactions, until comparatively recent times, has been somewhat neglected by the pure chemist.

In some of its aspects, the problem before us was discussed by one of my predecessors, Professor Hopkins, as also by Professor Macallum, but its importance will, I think, warrant my asking your indulgence for a further brief discussion. Permit me first to apologise for what may seem to some of those present to be an unnecessarily elementary treatment of certain points.

It is easy to realise that the molecules which are situated at the interface where two phases are in contact are subject to forces differing from those to which the molecules in the interior of either phase are subject. Consider one phase only, the molecules at its surface are exposed on the one side to the

influence of similar molecules; on the other side, they are exposed to the influence of molecules of a nature chemically unlike their own or in a different physical state of aggregation. The result of such asymmetric forces is that the phase boundary is the seat of various forms of energy not present in the interior of the phase. The most obvious of these is the surface energy due to the state of tension existing where a liquid or a gas forms one of the phases. It would lead us too far to discuss the mode of origin of this surface tension, except to call to mind that it is due to the attractive force of the molecules for one another, a force which is left partially unbalanced at the surface, so that the molecules here are pulled inwards. The tension is, of course, only the intensity factor of the surface energy, the capacity factor being the area of the surface. We see at once that any influence which alters the area of the surface alters also the magnitude of that form of energy of which we are speaking.

This is not the only way in which the properties of substances are changed at phase boundaries. The compressibility of a solvent, such as water, are altered, so that the solubilities of various substances in it are not the same as in the interior of the liquid phase. It is stated by J. J. Thomson that potassium sulphate is 60 per cent. more soluble in the surface film. The ways in which the properties of a solvent are changed are sometimes spoken of as 'lyotropic,' and they play an important part in the behaviour of colloids. We meet also with the presence of electrical charges, of positive or negative sign. These are due, as a rule, to electrolytic dissociation of the surface of one phase, in which the one ion, owing to its insolubility, remains fixed at the surface, while the opposite ion, although soluble, cannot wander away further than permitted by electrostatic attraction. Thus we have a Helmholtz double layer produced.

Before we pass on to consider how these phenomena intervene in physiological processes, there is one fact that should be referred to on account of its significance in connection with the contractile force of muscle. Surface tension is found to *decrease* as the temperature rises, or, as it is sometimes put, it has a negative temperature coefficient. This is unusual; but, if we remember that the interface between a liquid and its vapour disappears when the temperature rises to the critical point and, with it, of course, all phenomena at the boundary surface, the fact is not surprising that there is a diminution of these phenomena as the critical temperature is approached.

Perhaps that result of surface energy known as 'adsorption' is the one in which the conditions present at phase boundaries make themselves most frequently obvious. Since the name has been used somewhat loosely, it is a matter of some consequence to have clear ideas of what is meant when it is made use of. Unless it is used to describe a definite fact, it can only be mischievous to the progress of science.

Permit me, then, first to remind you of that fact of universal experience, known as the 'dissipation of energy,' which is involved in the second law of energetics. Free energy—that is, energy which can be used for the performance of useful work—is invariably found to diminish, if the conditions are such that this is possible. If we have, therefore, a system in which, by any change of distribution of the constituents, free energy can be decreased, such a change of distribution will take place. This is one form of the well-known 'Principle of Carnot and Clausius.'

Now, practically any substance dissolved in water lowers the surface tension present at the interface between the liquid and another solid or liquid phase with which it is in contact. Moreover, up to a certain limit, the magnitude of this effect is in proportion to the concentration of the solute. Therefore, as was first pointed out by Willard Gibbs, concentration of a solute at an interface has the effect of reducing free energy and will therefore occur. This is adsorption. As an example, we may take the deposition of a dye-stuff on the surface of charcoal, from which it can be removed again, unaltered, by appropriate means, such as extraction with alcohol. Charcoal plus dye may, if any satisfaction is derived from the statement, be called a compound. But, since its chemical composition depends on the concentration of the solution in which it was formed, it is much more accurate to qualify the statement by calling it an

'adsorption-compound.' Moreover, the suggestion that the union is a chemical one tends to deprive the conception of chemical combination of its characteristic quality, namely, the change of properties. Dye-stuff and charcoal are chemically unchanged by adsorption.

The origin of adsorption from surface tension is easily able to explain why it is less as the temperature rises, as we find experimentally. As we have just seen, surface tension diminishes with increase of temperature.

Let us next consider what will happen if the liquid phase contains in solution a substance which lowers surface tension and is also capable of entering into chemical reaction with the material of which the other, solid, phase consists. For example, a solution of caproic acid in contact with particles of aluminium hydroxide. On the surface of the solid, the concentration of the acid will be increased by adsorption and, in consequence, the rate of the reaction with it will be raised, according to the law of mass action. Further, suppose that the liquid phase contains two substances which react slowly with each other, but not with the solid phase. They will be brought into intimate contact with each other on the surface of the solid phase, their concentration raised and the rate of their interaction increased. One of the reagents may clearly be the solvent itself. But in all these cases the rate of the reaction cannot be expressed by a simple application of the law of mass action, since the active masses are not functions of the molecular concentrations, but of the surface of the phase boundaries. The application of these considerations to the problem of the action of enzymes and of heterogeneous catalysis in general will be apparent. That the action of enzymes is exerted by their surfaces is shown, apart from the fact that they are in colloidal solution, by the results of experiments made in liquids in which the enzymes themselves are insoluble in the usual sense, so that they can be filtered off by ordinary filter paper and the filtrate found to be free from enzyme. Notwithstanding this insolubility, enzymes are still active in these liquids. The statement has been found, up to the present, to apply to lipase, emulsin, and urease, probably to trypsin, and the only difficulty in extending it to all enzymes is that of finding a substrate soluble in some liquid in which the enzyme itself is not. That adsorption is a controlling factor in the velocity of enzyme action has been advocated by myself for some years, but it is not to be understood as implying that the whole action of enzymes is an 'adsorption phenomenon,' whatever may be the meaning of this statement. The rate at which the chemical reaction proceeds is controlled by the mass of the reagents concentrated on the surface of the enzyme phase at any given moment, but the temperature coefficient will, of course, be that of a chemical reaction.

The thought naturally suggests itself, may not the adsorption of the reacting substances on the surface of the enzyme suffice in itself to bring about the equilibrium at a greater rate, so that the assumption of a secondary chemical combination of a chemical nature between enzyme and substrate may be superfluous? I should hesitate somewhat to propose this hypothesis for serious consideration were it not that it was given by Faraday as the explanation of one of the most familiar cases of heterogeneous catalysis, namely, the union of oxygen and hydrogen gases by means of the surfaces of platinum and other substances. The insight shown by Faraday into the nature of the phenomena with which he was concerned is well known and has often caused astonishment. Now, this case of oxygen and hydrogen gases is clearly one of those called 'catalytic' by Berzelius. The fact that the agent responsible for the effect did not itself suffer change was clear to Faraday. I would also, in parenthesis, call attention to the fact that he correctly recognised the gold solutions which he prepared as suspensions of metallic particles, that is, as what we now call colloidal solutions. Although the systematic investigation of colloids, and the name itself, were due to Graham, some of the credit of the discovery should be given to the man who first saw what was their nature. Adsorption, again, was accurately described by Faraday, but without giving it a name

I confess that there are, at present, difficulties in the way of accepting concentration by adsorption as a complete explanation of the catalytic activities of enzymes. It is not obvious, for example, why the same enzyme should not be able to hydrolyse both maltose and saccharose, as it is usually expressed.

Another difficulty is that it is necessary to assume that the relative concentration of the components of the chemical system must be the same on the surface of the enzyme as it is in the body of the solution; in other words, the adsorption of each must be the same function of its concentration. Unless this were so, the equilibrium position on the enzyme surfaces, and therefore in the body of the solution, would be a different one under the action of an enzyme from that arrived at spontaneously or brought about by a homogeneous catalyst such as acid. This consideration was brought to my notice by Professor Hopkins and requires experimental investigation. We know, indeed, that in some cases there is such a difference in the position of the equilibrium position, for which various explanations have been suggested. But it would be a matter of some interest to know whether this difference has any relation to different degrees of adsorption of the components of the system.

At the same time, adsorption is under the control of so many factors, surface tension, electrical charge, and so on, that the possibilities seem innumerable. There are, moreover, two considerations to which I may be allowed to call your attention. Hardy has pointed out that it is probable that the increased rate of reaction at the interface between phases may be due, not merely to increased concentration as such, but that in the act of concentration itself molecular forces may be brought into play which result in a rise in chemical potential of the reacting substances. In the second place, Barger has shown that the adsorption of iodine by certain organic compounds is clearly related to the chemical composition of the surfaces of these substances, but that this relationship does not result in chemical combination nor in abolition of the essential nature of the process as an adsorption. It would appear that those properties of the surface, such as electric charge and so on, which control the degree of adsorption, are dependent on the chemical nature of the surface. This dependence need not cause us any surprise, since the physical properties of a substance, inclusive of surface tension, are so closely related to its chemical composition.

There is one practical conclusion to be derived from the facts already known with regard to enzymes. This is, that any simple application of the law of mass action cannot lead to a correct mathematical expression for the rate of reaction, although attempts of this kind have been made, as by Van Slyke. The rate must be proportional to the amount of substrate adsorbed, and this, again, is a function both of the concentration of the substrate and of that of the products. It is, then, a continuously varying quantity. Expressed mathematically, the differential equation for the velocity must be something of this kind:

$$\frac{dC}{dt} = KC^n$$

where n itself is an unknown function of C, the concentration of the substrate or products.

The hypothesis of control by adsorption gives a simple explanation of the exponential ratio between the concentration of the enzyme and its activity, which is found to be different numerically according to the stage of the reaction. At the beginning, it may be nearly unity, in the middle it is more nearly 0·5, as in the so-called 'square root law' of Schütz and Borissov, which is, however, merely an approximation. Simple explanations are also given of the fact that increasing the concentration of the substrate above a certain value no longer causes an increased rate of reaction. This is clearly because the active surface is saturated. Again, the effect of antiseptics and other substances which, by their great surface activity obtain possession of the enzyme surfaces, and thereby exclude to a greater or less degree the adsorption of the substrate, receives a reasonable account. In many cases, the depressant or favouring action of electrolytes, including acid and alkali, is probably due to aggregation or dispersion of the colloidal particles of the enzyme, with decrease or increase of their total surface. It is to be noted that such explanations are independent of any possible formation of an intermediate compound between enzyme and substrate, *after* adsorption has taken place.

There is a further way in which adsorption plays a part in the chemical processes of cells, including those under the influence of catalysts. It is a

familiar fact that the concentration of water plays a large part in the position of equilibrium attained in reversible reactions of hydrolysis and synthesis. A synthetic process is brought about by diminution of the effective concentration of water. There are, doubtless, means of doing this in the elaborate mechanisms of cell life, and, in all probability, it is by adsorption on surfaces, which are able to change their 'affinity' for water.

I pass on to consider briefly some other cases in which the phenomena at phase boundaries require attention.

Let us turn our gaze from the interior of the cell to the outer surface, at which it is in contact with the surrounding medium. From the nature of adsorption there can be no doubt that, if the cell or the surrounding liquid contains substances which decrease surface energy of any form, these constituents will be concentrated at the interface. There are many such substances to be found in cells, some of lipoid nature, some proteins, and so on. Further, the experiments of Ramsden have shown that a large number of substances are deposited in surface films in a more or less rigid or solidified form. We are thus led to inquire whether these phenomena do not account for the existence of the cell membrane, about which so much discussion has taken place. We find experimentally that there are facts which show that this membrane, under ordinary resting conditions, is impermeable to most crystalloids, including inorganic salts, acids, and bases. There is no other explanation of the fact that the salts present in cells are not only in different concentration inside from that outside, but that there may be absence of certain salts from one which are present in the other, as, for example, sodium in the plasma of the rabbit not in the corpuscles. Moreover, the experiments of Hoeber have shown that electrolytes are free in the cells, so that they are not prevented from diffusion by being fixed in any way. The mere assumption of a membrane impermeable to colloids only will not account for the facts, since, as I have shown in another place, this would only explain differences of concentration, but not of composition. The surface concentration of cell constituents readily accounts for the changes of permeability occurring in functional activity, since it depends on the nature of the cell protoplasm and chemical changes of many and various kinds occur in this system. If such be the nature of the cell membrane, it is evident that we are not justified in expecting to find it composed of lipoid or of protein alone. It must have a very complex composition, varying with the physiological state of the cell. Indeed, complex artificial membranes have been prepared having properties very similar to that of the cell.

This view that the membrane is formed by surface condensation of constituents of the cell readily accounts for the changes of permeability occurring in functional activity, since its composition depends on that of the cell protoplasm, and chemical changes of various kinds take place in this system, as it is scarcely necessary to remind you. In fact, the cell membrane is not to be regarded as an independent entity, but as a working partner, as it were, in the business of the life of the cell. In the state of excitation, for example, there is satisfactory evidence that the cell membrane loses its character of semi-permeability to electrolytes, &c. This statement has been shown to apply to muscle, nerve, gland cells, and the excitable tissues of plants, as well as to unicellular organisms. We shall see presently how this fact gives a simple explanation of the electrical changes associated with the state of activity.

If, then, the cell membrane is a part of the cell system as a whole, it is not surprising to find that substances can affect profoundly, although reversibly, the activities of the cell, even when they are unable to pass beyond the outer surface. The state of dynamic equilibrium between the cell membrane and the rest of the cell system is naturally affected by such means, since the changes in the one component involve compensating ones in the other. Interesting examples of such actions are numerous. I may mention the effect of calcium ions on the heart muscle, the effect of sodium hydroxide on oxidation in the eggs of the sea-urchin, and that of acids on the contraction of the jelly-fish. Somewhat puzzling are those cases in which drugs, such as pilocarpine and muscarine, act only during their passage through the membrane and lose their effect when their concentration has become equal inside and outside the cell.

The work of Dale on anaphylaxis leads him to the conclusion that the

phenomena shown by sensitised plain muscle can most reasonably be explained by colloidal interaction on the surface of the fibres. The result of this is increased permeability and excitation resulting therefrom.

I referred previously to the electrical change in excitable tissues and its relation to the cell membrane. It was, I believe, first pointed out by Ostwald and confirmed by many subsequent investigators, that in order that a membrane may be impermeable to a salt it is not a necessary condition that it shall be impermeable to both of the ions into which this salt is electrolytically dissociated. If impermeable to one only of these ions, the other, diffusible, ion cannot pass out beyond the point at which the osmotic pressure due to its kinetic energy balances the electrostatic attraction of the oppositely charged ion, which is imprisoned. There is a Helmholtz double layer formed at the membrane, the outside having a charge of the sign of the diffusible ions, the inside that of the other ions. Now, suppose that we lead off from two places on the surface of a cell having a membrane with such properties to some instrument capable of detecting differences of electrical potential. It will be clear that we shall obtain no indication of the presence of the electrical charge, because the two points are equipotential, and we cannot get at the interior of the cell without destroying its structure. But if excitation means increased permeability, the double layer will disappear at an excited spot owing to indiscriminate mixing of both kinds of ions and we are then practically leading off from the interior of the cell, that is, from the internal component of the double layer, while the unexcited spot is still led off from the outer component. The two contacts are no longer equipotential. Since we find experimentally that a point at rest is electrically positive to an excited one, the outer component must be positive, or the membrane is permeable to certain cations, impermeable to the corresponding anions. Any action on the cell such as would make the membrane permeable, injury, certain chemical agents, and so on, would have the same effect as the state of excitation. If we may assume the possibility of degrees of permeability, the state of inhibition might be produced by *decrease* of permeability of the membrane of a cell, which was previously in a state of excitation owing to some influence, inherent in the cell itself or coming from the outside. This manner of accounting for the electromotive changes in cells is practically the same as that given by Bernstein.

It will be found of interest to apply to secretory cells the facts to which I have directed your attention. If we suppose that the setting into play of such cells is associated with the production of some osmotically active substance, together with abolition of the state of semi-permeability of the membrane covering the ends of the cells in relation with the lumen of the alveolus of the gland, it is plain that water would be taken up from the lymph spaces and capillaries and escape to the duct, carrying with it the secretory products of the cells. This process would be continuous as long as osmotically active substances were formed. Such a process has been shown by Lepeshkin to occur in plants and we have also evidence of increased permeability during secretory activity in the gland cells of animals. From what has been said previously, it is evident that electrical differences would show themselves between the permeable and semi-permeable ends of such cells, as has been found to be the case.

As a modifiable structure, we see the importance of such a membrane as that described if it takes part in the formation of the synapse between neurones. The manifold possibilities of allowing passage to states of excitation or inhibition and of being affected by drugs will be obvious without further elaboration on my part.

Enough has already been said, I think, to show the innumerable ways in which phenomena at phase boundaries intervene in physiological events. Indeed, there are very few of these, if any, in which some component or other is not controlled by the action of surfaces of contact. But there is one especially important case to which I may be allowed to devote a few words in conclusion. I refer to the contractile process of muscle. It has become clear, chiefly through the work of Fletcher, Hopkins, and A. V. Hill, that what is usually called muscular contraction consists of two parts. Starting from the resting muscle, we find that it must have a store of potential energy, since we can make it do work when stimulated. After being used in this way, the store must be replenished, since

energy cannot be obtained from nothing. This restoration process is effected by an independent oxidation reaction, in which carbohydrate is burnt up with the setting free of energy which is made use of to restore the muscle to its original state. Confining our attention for the moment to the initial, contractile, stage, the essential fact is the production of a certain amount of energy of tension, which can either be used for the performance of external work or be allowed to become degraded to heat in the muscle itself. It was Blix who first propounded the view that the amount of this energy of tension is related to the magnitude of certain surfaces in the muscle fibres. But the fact was demonstrated in a systematic and quantitative manner by A. V. Hill. He showed, in fact, that the amount of energy set free in the contractile process is directly related to the length of muscle fibres during the development of the state of tension. In other words, the process is a surface phenomenon, not one of volume, and is directly proportional to the area of certain surfaces arranged longitudinally in the muscle. This same relationship has been shown by Patterson and Starling to hold for the ventricular contraction of the mammalian heart and by Kosawa for that of the cold-blooded vertebrate. It appears that all the phenomena connected with the output of blood by the heart can be satisfactorily explained by the hypothesis that the energy of the contraction is regulated by the *length* of the ventricular fibres during the period of development of the contractile stress. The degree of filling at the moment of contraction is thus the determining factor.

That surface tension itself may be responsible for the energy given off in muscular contraction was first suggested by Fitzgerald in 1878, and it seems, from calculations made, that changes at the contact surface of the fibrillæ with the sarcoplasm may be capable of affording a sufficient amount. The difficulties in deciding the question are great, but, in addition to the facts mentioned, there is other interesting evidence at hand. It has been shown, by Gad and Heymans, by Bernstein and others, that the contractile stress produced by a stimulus has a negative temperature coefficient. Within the limits of temperature between which the muscle can be regarded as normal, this stress is the greater the lower the temperature. The same statement was shown by Weizsäcker (working with A. V. Hill) to hold for the heat developed in the contractile stage. Now, of all the forms of energy possibly concerned, that associated with phase boundaries is the only one with a negative temperature coefficient. Another aspect of this relation to temperature is the well-known increase of the tonus of smooth muscle with fall in temperature.

It is tempting to bring into relation with the change in surface tension the production of lactic acid. In fact, this idea was put into a definite statement by Haber and Klemensievich in 1909 in a frequently quoted paper on the forces present at phase boundaries. The production of acid is stated to alter the electrical forces at this situation. This electrical charge involves a change of surface tension, and it is this change of surface tension which brings about the mechanical deformation of the muscle. Mines also has brought forward good evidence that the production of lactic acid is responsible for the change of tension. As to how the lactic acid is set free, and of what nature the system of high potential present in muscle may be, we require much more information. The absence of evolution of carbon dioxide when oxygen is not present shows that no oxidation takes place in the development of tension. There are other difficulties also in supposing that this system present in resting muscle is of a chemical nature. If the energy afforded by the oxidation of carbohydrate in the recovery stage is utilised for the formation of another chemical system with high energy content, the theory of coupled reactions indicates that there must be some component common to both systems. It is difficult to see what component of the muscle system could satisfy the conditions required. On the whole, some kind of system of a more physical nature seems the most probable. If it be correct that the oxidation of substances other than carbohydrate, fat for example, can afford the chemical energy for muscular contraction, as appears from the results of metabolism experiments, a further difficulty arises in respect to a coupled reaction. But the question still awaits investigation.

On the whole, I think that we may conclude that more study of the phenomena at phase boundaries will throw light on many problems still obscure. It would

probably not be going too far to say that the peculiarities of the phenomena called 'vital' are due to the fact that they are manifestations of interchange of energy between the phases of heterogeneous systems. It was Clerk Maxwell who compared the transactions of the material universe to mercantile operations in which so much credit is transferred from one place to another, energy being the representative of credit. There are many indications that it is just in this process of change of energy from one form to another that special degrees of activity are to be observed. Such, for example, are the electrical phenomena seen in the oxidation of phosphorus or benzaldehyde, and it appears that, in the photo-chemical system of the green plant, radiant energy is caught on the way, as it were, to its degradation to heat, and utilised for chemical work. In a somewhat similar way, it might be said that money in the process of transfer is more readily diverted, although perhaps not always to such good purpose as in the chloroplast. Again, just as in commerce money that is unemployed is of no value, so it is in physiology. Life is incessant change or transfer of energy, and a system in statical equilibrium is dead.

British Association for the Advancement of Science.

SECTION K: MANCHESTER, 1915.

ADDRESS

TO THE

BOTANICAL SECTION

BY

PROFESSOR W. H. LANG, F.R.S.,

PRESIDENT OF THE SECTION.

SINCE I am not a visitor to our place of meeting it is my privilege to extend to the members of this Section a special welcome on behalf of our Botanical Department.

The war has diminished for the time the number of those engaged in botanical work. I shall not attempt any mention of those botanists who are serving with the forces, are assisting in the training of recruits, or are otherwise playing their parts in the service of the country. Some have been wounded, but, we rejoice to know, recovered. We have, however, to express our sorrow, tempered with pride, at the cutting short of the promising botanical careers of Mr. Laidlaw and Mr. Lee, who have been killed at the front.

While we have no group of foreign guests, as at the last Manchester meeting, we owe to the war the presence of a distinguished Belgian botanist, Professor Julius MacLeod, and shall hear some of the results of investigations he has made while in England. In welcoming him to this meeting we hope that he may soon be able to return to his own University of Ghent when the invaders are expelled from his country.

Phyletic and Causal Morphology.

I propose to deal with some aspects of the study of plant-morphology. In doing so I shall not accept any definition of morphology that would separate it artificially from other departments of botany. I regard the aim of plant-morphology as the study and scientific explanation of the form, structure, and development of plants. This abandons any sharp separation of morphology and physiology and claims for morphology a wider scope than has been customary for the past fifty years. During this period the problem of morphology has been recognised as being 'a purely historical one,' 'perfectly distinct from any of the questions with which physiology has to do,' its aim being 'to reconstruct the evolutionary tree.' The limitation of the purpose of morphological study, expressed in these phrases from the admirable addresses delivered to this Section by Dr. Scott and Professor Bower some twenty years ago, was due to the influence of the theory of descent. I fully recognise the interest of the phyletic ideal, but am unable to regard it as the exclusive, or perhaps as the most important, object of morphological investigation. To accept the limitation of morphology to genealogical problems is inconsistent with the progress of this branch of study before the acceptance of the theory of descent, and leaves out many of the most important problems that were raised and studied by the earlier morphologists.

In the history of morphology, after it had ceased to be the handmaid of the systematic botany of the higher plants, we may broadly distinguish an idealistic period, a developmental period, and a phyletic period. The period of developmental morphology, the most fruitful and the most purely inductive in our science, was characterised by an intimate connection between morphological and physiological work. Among its contributions were studies of development or 'growth histories' of whole plants and their members. These were carried out, in part at least, in order to investigate the nature of development, and such general problems found their expression at the close of the period in the 'Allgemeine Morphologie' of Hofmeister. The 'Origin of Species' took some years before it affected the methods and aims of botanical work. Then its effect on morphology was revolutionary and, as in all revolutions, some of the best elements of the previous régime were temporarily obscured. This excessive influence of the theory of descent upon morphology did not come from Darwin himself but from his apostle Haeckel, who gave a very precise expression to the idea of a genealogical grouping of animals and plants, illustrated by elaborate hypothetical phylogenetic trees. Such ideas rapidly dominated morphological work, and we find a special 'phylogenetic method' advocated by Strasburger.[1] The persistence of the phyletic period to the present time is shown not only in the devotion of morphology to questions of relationship but in the attempts made to base homologies upon descent only. Lankester's idea of homogeny can be traced to the influence of Haeckel, and nothing shows the consistency of phyletic morphology to its clear but somewhat narrow ideal so plainly as the repeated attempts to introduce into practice a sharp distinction between homogeny and homoplasy.

Professor Bower, in his Address last year and in other papers, has dealt illuminatingly with the aims and methods of phyletic morphology. I need only direct attention to some aspects of the present position of this, which bear on causal morphology. The goal of phyletic morphology has throughout been to construct the genealogical tree of the Vegetable Kingdom. In some ways this seems farther off than ever. Phyletic work has been its own critic, and the phylogeny of the genealogical tree, since that first very complete monophyletic one by Haeckel, affords a clear example of a reduction series. The most recent and reliable graphic representations of the inter-relationships of plants look more like a bundle of sticks than a tree. Consider for a moment our complete ignorance of the inter-relationships of the Algæ, Bryophyta, and Pteridophyta. Regarding the Algæ we have no direct evidence, but the comparative study of existing forms has suggested parallel developments along four or more main lines from different starting-points in a very simple unicellular ancestry. We have no clue, direct or indirect, to the ancestral forms of the Bryophyta, and it is an open question whether there may not be as many parallel series in this group as in the Algæ. The Pteridophyta seem a better case, for we have direct evidence from fossil plants as well as the comparison of living forms to assist us. Though palæobotany has added the Sphenophyllales to the existing groups of Vascular Cryptogams and has greatly enlarged our conceptions of the others, there is no proof of how the great groups are related to one another. As in the Bryophyta, they may represent several completely independent parallel lines. There is no evidence as to what sort of plants the Pteridophyta were derived from, and in particular none that relates them to any group of Bryophyta or Algæ. I do not want to labour the argument, but much the same can be said of the seed-plants, though there is considerable evidence and fairly general agreement as to some Gymnosperms having come from ancient Filicales. The progress of phyletic work has thus brought into relief the limitations of the possible results and the inherent difficulties. As pointed out by Professor Bower, we can hope for detailed and definite results only in particularly favourable cases, like that of the Filicales.

The change of attitude shown in recent phyletic work towards 'parallel

[1] The claims of this phylogenetic method were at once criticised by Braun, in a form that deserves careful study to-day (*Monatsb. d. K. Akad. Wiss.* Berlin. 1875, p. 243 ff.).

developments in phyla which are believed to have been of distinct origin'[2] is even more significant. Professor Bower spoke of the prevalence of this as an 'obstacle to success,' and so it is if our aim is purely phyletic. In another way the demonstration of parallel developments constitutes a positive result of great value. Thus Professor Bower's own work has led to the recognition of a number of series leading from the lower to the higher Filicales. By independent but parallel evolutionary paths, from diverse starting-points in the more ancient Ferns, such similarity has been reached that systematists have placed the plants of distinct origin in the same genus. In these progressions a number of characters run more or less clearly parallel, so that the final result appears to be due 'to a phyletic drift that may have affected similarly a plurality of lines of descent.' This conclusion, based on detailed investigation, appears to me to be of far-reaching importance. If a 'phyletic drift' in the Ferns has resulted in the independent and parallel origin of such characters as dictyostely, the mixed sorus, and the very definite type of sporangium with a vertical annulus and transverse dehiscence, the case for parallel developments in other groups is greatly strengthened. The interest shifts to the causes underlying such progressive changes as appear in parallel developments and the problem becomes one of causal morphology rather than purely historical.

The study of parallel developments would, indeed, seem likely to throw more light on the morphology of plants than the changes traced in a pure phyletic line, for it leads us to seek for common causes, whether internal or external. We cease to be limited in our comparisons by actual relationship, or forbidden to elucidate the organisation in one group by that which has arisen independently in another. Similarly the prohibition against comparing the one generation in the life-cycle with the other falls to the ground, quite apart from any question of whether the alternation is homologous or antithetic. The methods of advance[3] and the causal factors concerned become the important things, and if, for example, light is thrown on the organisation of the fern-plant by comparison with the gametophyte of the moss, so much the better. This, however, is frankly to abandon phylogeny as 'the only real basis of morphological study'[4] and with this any attempt to base homology on homogeny. Many of the homologies that exist between series of parallel development are what have been happily termed homologies of organisation; these are sometimes so close as to result in practical identity, at other times so distinct as to be evident homoplasies. The critical study of homologies of organisation over as wide an area as possible becomes of primary interest and importance.

Since about the beginning of the present century a change of attitude towards morphological problems has become more and more evident in several ways. It seems to be a phyletic drift affecting simultaneously a plurality of lines of thought. The increasing tendency to look upon problems of development and construction from a causal standpoint is seen in the prominence given to what may be termed developmental physiology and also in what Goebel has called Organography.[5] These deal with the same problems from different sides and

[2] This and other quotations in this paragraph are from Professor Bower's address to Section K in Australia (*Brit. Assoc. Rep.* 1914).

[3] In this connection it is of interest to remember that Professor Bower has always laid stress on the importance of studying methods of advance and has regarded in this way examples which some other morphologists have used to form an actual series. His use of the bryophyte sporogonium in explaining the origin of the sporophyte is a case in point. (Cf. *Annals of Botany*, vol. viii. p. 344.)

[4] Strasburger, *Text Book of Botany*, p. 9. It would be truer to say that morphology has been the basis of phylogeny. If each is to be the basis of the other. the building can hardly progress!

[5] The special meaning attached by Goebel to Organography is difficult to ascertain and has undergone a fundamental change between the first and second editions of the *Allgemeine Organographie*. In the second edition (p. 8) the dependence of construction on function is regarded as open to question and 'in specific cases as untenable (p. 39). The justification for an Organography instead of a General Morphology would thus really disappear.

neither formulates them as they appear to the morphologist. Together with genetics, they indicate the need of recognising what I prefer to call General or Causal Morphology.

The problems of causal morphology are not new, though most of them are still unsolved and are difficult to formulate, let alone to answer. As we have seen, they were recognised in the time of developmental morphology, though they have since been almost wholly neglected by morphologists. So far as they have been studied during the phyletic period, it has been from the physiological rather than the morphological side. Still such problems force themselves upon the ordinary morphologist, and it is from his position that I venture to approach them. I willingly recognise, however, that causal morphology may also be regarded as a department of plant-physiology. In development, which is the essential of the problem, the distinction between morphology and physiology really disappears, even if this distinction can be usefully maintained in the study of the fully developed organism. We are brought up against a fact which is readily overlooked in these days of specialisation, that Botany is the scientific study of plants.

General morphology agrees with physiology in its aim being a causal explanation of the plant and not historical. Its problems would remain if the phyletic history were before us in full. In the present state of our ignorance, however, we need not be limited to a physico-chemical explanation of the plant. Modern physiology rightly aims at this so far as possible, but, while successful in some departments, has to adopt other methods of explanation and analysis in dealing with irritability. It is even more obvious that no physico-chemical explanation extends far enough to reach the problems of development and morphological construction. The morphologist must therefore take the complicated form and its genesis in development and strive for a morphological analysis of the developing plant. This is to attack the problem from the other side and to work back from the phenomena of organisation toward concepts of the nature of the underlying substance.

It is to these questions of general morphology with a causal aim (for causal morphology, though convenient, is really too ambitious a name for anything we yet possess) that I wish to ask your attention. All we can do at first is to take up a new attitude towards our problems, and to gather here and there hints upon which new lines of attack may be based. This new attitude is, however, as I have pointed out, a very old one, and in adopting it we re-connect [6] with the period of developmental morphology. Since the limited time at my disposal forbids adequate reference to historical details, and to the work and thought of many botanists [7] in this field, let me in a word disclaim any originality in trying to express in relation to some morphological problems what seems to me the significant trend, in part deliberate and in part unconscious, of morphology at present. The methods available in causal morphology are the detailed study in selected plants of the normal development and its results, comparison over as wide an area as possible with special attention to the essential correspondences (homologies of organisation) arrived at independently, the study of variations, mutations, and abnormalities in the light of their development, and ultimately critical experimental work. This will be evident in the following attempt to look at some old questions from the causal point of view. I shall take them as suggested by the Fern without confining my remarks to this. The Fern presents all the main problems in the morphology of the vegetative organs of the higher plants, and what little I have to say regarding the further step to the seed-habit will come as a natural appendix to its consideration.

Individual Development.

Twice in its normal life-history the Fern exhibits a process of development starting from the single cell and resulting in the one case in the prothallus and in the other in the fern-plant. For the present we may treat these two

[6] It is no mere coincidence that modern genetics is due to the re-discovery of the work of an investigator of this period.

[7] It will be sufficient to mention the names of Knight, Naegeli, Leitgeb, Hofmeister, Vöchting, Sachs, Klebs, and Goebel as the earlier workers on this line.

stages in the life-history as individuals, their development presenting the same general problems as a plant of, say, *Fucus* or *Enteromorpha*, where there is no alternation of generations. How is the morphologist to regard this process of individual development?

In the first place we seem forced to regard the specific distinctness as holding for the germ as well as the resulting mature plant, however the relation between the germ-cell and the characters of the developed organism is to be explained. We start thus with a conception of specific substance,[8] leaving it quite an open question on what the specific nature depends. This enables us to state the problem of development freed from all considerations of the ultimate uses of the developed structure. The course of development to the adult condition can be looked upon as the manifestation of the properties of the specific substance under certain conditions. This decides our attitude as morphologists to the functions of the plant and to teleology. Function does not concern us except in so far as it is found to enter as a causal factor into the process of development. Similarly until purpose can be shown to be effective as a causal factor it is merely an unfortunate expression for the result attained.

Let me remind you also that the individual plant, whether it be unicellular, cœnocytic or multicellular, may behave as a whole at all stages of its development. We see this, for instance, in the germination of *Œdogonium*, in the germination and subsequent strengthening of the basal region in *Fucus* or *Laminaria*, in the moss-plant or fern-plant, or in a dicotyledonous tree. A system of relations is evident in the plant, expressed in the polarity and the mutual influences of the main axis and lateral branches, as well as in the influences exerted on the basal region by the distant growing parts. We thus recognise, in its most general form, the correlation of parts, a concept of proved value in botany.

To some the expression of the observed facts in this way may appear perilously mystical. I do not think so myself. It is true that the nature of the specific substance and of the system of relations is unknown to us, but it is regarded as a subject for scientific inquiry and further explanation. To recognise fully the complexity of the substance of the plant is not, however, a step towards neo-vitalism, but is perhaps our best safeguard against the dangers of this.

The wholeness of the individual, together with important phenomena of regeneration, has suggested the conclusion that something other than physico-chemical or mechanical laws are concerned in the development of the organism. To this something Driesch applies the name entelechy. Without discussing the vitalistic philosophy of the organism, or other modern phases of philosophic thought that treat life as an entity, it seems worth while to point out that they are based mainly on the consideration of animal development. It would be interesting to inquire into the difficulties that are met with in applying such views to plants, where regeneration in one form or another is the rule rather than the exception and often does not lead to restitution of the individual. Causal morphology can recognise phenomena of development and of the individual, which are at present beyond physico-chemical explanation, and try to attack them by any methods of investigation that seem practicable, without begging the main question at the outset and then proceeding deductively. To assume any special inner director of development, be it entelechy or vital force, is to cut the knot that may ultimately be untied.

The previous experience of botany in the time of nature-philosophy may well make us cautious of solving our difficulties by the help of any new biological philosophy. On the other hand, co-operation between biology and philosophic thought is highly desirable. In this connection I should like to refer to an idea contained in Professor Alexander's paper on the Basis of Realism.[9] He

[8] The more general concept of specific substance, which avoids hypotheses of heredity, seems preferable for our purpose to that of idioplasm or even of specific cell, since it leaves open the possibility of some properties of the plant being generalised in the protoplasm and not to be explained by the mutual relation of cells; it also covers the case of cœnocytic plants.

[9] Royal British Academy, Jan. 28, 1914, p. 12.

suggests that there is only one matrix from which all qualities arise, and that (without introducing any fresh stuff of existence) the secondary qualities, life, and at a still higher level mind, emerge by some grouping of the elements within the matrix. The development of this idea as it applies to life would appear to offer a real point of contact between inductive biological work and philosophy.

To return to our plant, its development, with increase in size and progressive complexity of external form and internal structure, must be considered. The power of continued development possessed by most plants and wanting in most animals makes comparison between the two kingdoms difficult. That there is no fundamental difference between the continued and the definitely limited types of embryogeny is, however, shown by plants themselves. The bryophyte sporogonium is a clear example of the latter, while the fern sporophyte is one of many examples of the former. A difference less commonly emphasised is that in the sporogonium (as in the higher animals) the later stages of development proceed by transformation of the whole of the embryo into the mature or adult condition; in the fern-plant the apical development results in successive additions of regions which then attain their mature structure by transformation of the meristematic tissue.

These distinctions are of some importance in considering the generalisation originally founded on animal development and known as the biogenetic law. That 'the ontogeny is a concise and compressed recapitulation of the phylogeny' is essentially a phyletic conception. It has been more or less criticised and challenged by some distinguished zoologists, and has always been difficult to apply to plants. If we avoid being prejudiced by zoological theory and results, we do not find that the characters of the embryos of plants have given the key to doubtful questions of phylogeny. What help do they give us, for instance, in the Algæ or the Vascular Cryptogams? The extension of the idea of recapitulation to the successively formed regions of the seedling plant requires critical examination; if admitted, it is at any rate something different from what the zoologist usually means by this. The facts—as shown, for instance, in a young fern-plant—are most interesting, but can perhaps be better looked at in another way. Development is accompanied by an increase in size of the successively formed leaves and portions of stem, and the process is often cumulative, going on more and more rapidly as the means increase until the adult proportions are attained. The same specific system of relations may thus find different expression in the developing plant as constructive materials accumulate. I do not want to imply that the question is merely a quantitative one; quality of material may be involved, or the explanation may lie still deeper. The point is that the progression is not a necessary one due to some recapitulative memory.

There are some other classes of facts, clearly cognate to normal individual development, that seem to require the causal explanation. I may mention three: (1) Vegetatively produced plants (from bulbils, gemmæ, &c.) tend in their development to pass through stages in elaboration similar to young plants developing from a spore or zygote. The similarities are more striking the smaller the portion of material from which a start is made. (2) Branches may repeat the stages in ontogeny more or less completely also in relation to differences in the nutritive conditions. (3) In the course of continued development there may be a return to the simpler form and structure passed through on the way to the more complex. These cases of parallels to, or reversals of, the normal ontogenetic sequence suggest explanation on causal lines but are difficulties in the way of phyletic recapitulation; the first two cases can be included under this, while the third seems definitely antagonistic. On the whole it may be said that recapitulation cannot be accepted for plants without further evidence, and that preliminary inquiry disposes us to seek a deeper and more fruitful method of explaining the facts of development.

The development of most plant-individuals starts from a single cell, and when we compare mature forms of various grades of complexity the unicellular condition is also our usual starting point. What is not so generally recognised or emphasised is the importance of the filament as the primitive construction-form of most plants. I do not use the word primitive in a phyletic sense, nor in the sense of an ideal form, but to indicate a real stage in independent progressions underlying many homologies of organisation. I cannot

develop this fully here, but wide comparison of independent lines of advance suggests that the main types of progress in complexity of the plant-body [10] have involved the elaboration of the single filament with apical growth and with subordinated 'branches.' It is generally recognised that various groups of Algæ show how a solid multicellular axis may come about not only by the further partition of the segments of the apical cell but by the congenital cortication of a central filament or the congenital condensation of the subordinated 'branches' on to the central axis. The Algæ further show the change from the dome-shaped apical cell of a filament to the sunken initial cell with two, three, or four sides. The central filament then only appears, if at all, as a subsequent differentiation in the tissue, and the segments serially cut off from the apical cell may or may not bear projecting hair-shoots or 'leaves.' The Algæ thus attain in independent lines a construction corresponding to that of the plant in Liverworts and Mosses. In the various parallel series of Bryophyta the filament is not only more or less evident in the ontogeny but may be regarded as the form underlying both thallus and shoot, between which on this view there is no fundamental distinction. The sporogonium also can be readily regarded as an elaborated filament. While the same interpretation of the fern-prothallus will readily be granted, to think of the fern-plant as the equivalent of an elaborated filament may appear far-fetched. So far from this being the case, I believe that it will be found helpful in understanding the essential morphology of the shoot. In a number of Vascular Cryptogams and Seed-plants, there is actually a filamentous juvenile stage, the suspensor, while the growth by a single apical cell is essentially the same in the fern as in the moss and some Algæ.

There follows from this a natural explanation of the growth by a single initial cell so commonly found in plants. The apical cell appears to be the one part of the massive plant-body (for instance, of *Laurencia*, a moss, or a fern) that persists as a filament; it is a filament one cell long. It may be replaced by a group of initial cells, as we see in some Algæ, Liverworts, and Pteridophyta, and this leads naturally to the small-celled meristems found in most Gymnosperms and Angiosperms. The filamentous condition is then wholly lost, though the system of relations and especially the polarity is maintained throughout all the changes in the apical meristem.

I feel confirmed in regarding the construction of the sporophyte in this fashion by the fact that it fits naturally with the conclusions resulting from the masterly comparative treatment of the embryology of the Vascular Cryptogams by Professor Bower.[11] These are (1) the primary importance of the longitudinal axis of the shoot, the position of the first root and the foot being variable; (2) the constancy of the position of the stem-apex near the centre of the epibasal half of the embryo; (3) the probability that embryos without suspensors have been derived from forms with suspensors, without any example of the converse change. These and other related facts seem to find their morphological explanation in the shoot of the sporophyte being the result of the elaboration of a filament.

The Construction of the Shoot.

The view to which we are thus led is that the uniaxial shoot is a complex whole, equivalent to the axial filament together with its congenitally associated subordinated 'branches.' This applies to the multicellular plant-bodies found in various independent lines of Algæ and Bryophyta, whether they have definite projecting appendages of the nature of leaves or not. The discarding of the distinction between thallus and shoot, which in practice has proved an unsatisfactory one, is no great loss. Even taking the word in the narrower sense of a stem with distinct leaves, the shoots in Algæ, Liverworts, and Mosses, though

[10] There are other lines of progression in complexity, the most important being the formation of a plant-body by the co-ordinated growth of a number of filaments. This line of advance is seen in many Algæ, in the larger Fungi, and in the Lichens. Though it raises morphological questions of great interest, it is here left out of consideration as not bearing directly on the organisation of the higher plants.

[11] *Land Flora*, chap. xlii.

admittedly independent developments, exhibit an essential correspondence amounting to a homology of organisation. The resemblances are not analogies, for it is doubtful whether the 'leaves' in the different cases correspond in function. The comparison of the shoot of the sporophyte of a Vascular Cryptogam with, for example, the shoot of the Moss seems equally justifiable. It is only forbidden by strict phyletic morphology, which for our purpose has no jurisdiction. The general agreement as regards the leaf-arrangement between the Ferns and the Bryophyta suggests that similar laws will be found to hold in the shoot of both gametophyte and sporophyte. Apart from plagiotropic shoots, there is a constructionally dorsiventral type of fern-rhizome. The leaves of this alternate as in the leafy Liverworts, while the radial type of fern corresponds to the moss-shoot. It is significant that the early leaves of radially constructed ferns usually exhibit a divergence of $\frac{1}{3}$ in the seedling, passing higher up the stem into more complicated arrangements, and the same is the case in mosses. I must not enter into questions of phyllotaxy, but may remark on the hopefulness of attacking it from the study of the simpler shoots of Algæ and Bryophyta rather than, as has usually been done, beginning with the Flowering Plants.

In some ferns (the striking example being *Ceratopteris*) the relation between the segmentation of the apical cell and leaf-production is as definite as in the moss, each segment giving rise to a leaf. This may hold more widely for ferns than is at present demonstrated, and the question deserves thorough re-investigation to ascertain the facts independently of any theoretical views. That the coincidence of the segmentation of the shoot expressed by the leaf-arrangement and the segmentation of an apical cell is not a necessary one is, however, clearly shown in other ferns, and is obvious in the case of shoots with a small-celled meristem. The two segmentations appear to be determined by some deeper system of relations, which may also be manifested in a cœnocytic plant-body.

In the complication of the uniaxial shoot introduced by branching also there seems to be an advantage in a wide area of comparison. The question most often discussed concerns dichotomous and monopodial branching. If the details of development are to be taken into consideration, the term 'dichotomy' has usually been very loosely applied. Apparent dichotomy, the continuation of one shoot by two equally strong ones, is fairly common. But in most cases investigated in detail the branching seems to be really monopodial and the forking due to the equally strong development of a lateral branch close to the main apex, not to the division of the latter. In plants growing by a single initial cell almost the only case of strict dichotomy known is the classic one of *Dictyota*. The branching of the ferns has been the subject of numerous investigations, but there is a great lack of developmental data. Usually the branches stand in some definite relation to the leaves of the shoot, behind, to one side or on the leaf-base itself, the most interesting but least common case being when the branch is in an axillary position. When the mature shoot only is considered, it is possible to argue for the derivation of monopodial branching from dichotomy or the converse. Even the facts obtainable from the mature plant, however, point to the dichotomous branching being a modification of the monopodial, the opposite view appearing to land us in difficulties regarding the morphology of the main shoot.[12] It is unlikely that a dichotomy involving the division of the apical cell occurs in the fern-shoot, and comparison with the Bryophyta confirms the suspicion that the cases of dichotomy are only apparent.

In considering the construction of the shoot we are at present limited to comparison of the normal structure and development. The system of relations in the shoot of the fern, affecting in the first place the distribution of the leaves and secondly that of the branches, appears, however, to be of the same nature as in the independently evolved shoots of Bryophyta and Algæ. A morphological analysis based on the simpler examples may lead on to the experimental investigation of the common construction. The relation that exists between the general construction and the vascular anatomy offers a special and more

[12] The conclusion arrived at by Schoute, that the angular leaf in 'dichotomous' branching and the subtending leaf in monopodial branching are equivalent, supports this view. *Recueil des Travaux botaniques Néerlandais*, vol. xi.

immediately hopeful problem. Here also, in considering the fern, we are assisted by homologies of organisation in other Vascular Cryptogams and in the more complex Bryophyta, though the Algæ are of little help.

In few departments of botany has our knowledge increased so greatly and become so accurate as in that of vascular anatomy. The definiteness of the structures concerned and the fact that they have been almost as readily studied in fossil as in living plants has led to this. Not less important have been the clear concepts first of the bundle system and later of the stele under which the wealth of fact has been brought. Great progress has been made under the influence of phyletic morphology, and anatomy has adopted further conventions of its own and tended to treat the vascular system as if it had an almost independent existence in the plant. The chief method employed has been the comparative study of the mature regions, of necessity in the fossils and by choice in the case of existing plants. I do not, of course, mean to say that we are ignorant of the development of the vascular system, but the variety in it has not been adequately studied in the light of apical development. A gap in our knowledge usually comes between the apical meristem itself and the region with a developed vascular system. It is in this intermediate region that the real differentiation takes place and the arrangement of the first vascular tracts is then modified by unequal extension of the various parts. The apical differentiation requires separate study for each grade of complexity of the vascular system even in the same plant.

If we look at the vascular system, not as if it had an independent existence nor from the phyletic point of view, but as a differentiation taking place within the body of the individual plant, we can inquire as to the causal factors in the [illegible] A deeper insight into the nature of the stele may be obtained by regarding it as the resultant of a number of factors, as part of the manifestation of the system of relations in development. The first step towards this is the critical consideration of normal developing plants, but so long as the causal influences in the developing substance of a plant remain unchanged the resulting vascular structure will remain constant. Our hope of advance lies in the study of cases where these influences are modified. Herein lies the value of abnormalities, of natural experiments, and the results of experimental interference. Possible influences that have at various times been suggested are functional stimuli, the inductive influence of the older pre-formed parts on the developing region, and formative stimuli of unknown nature proceeding from the developing region. The functional stimuli do not come into play at the time of laying down the vascular tracts, though they may have importance in their maintenance later; the inductive influence of the anatomy of older regions is excluded in the first differentiation of the vascular system in an embryo; we are thus led to attach special importance to the detection of the action of formative stimuli proceeding from the young developing primordia. We have further to take external stimuli into account, though these must act by influencing the internal system of relations.

Time will not permit of reference to the scattered literature bearing on this subject, but it may make the reality of such formative stimuli a little clearer if I refer to some examples that have turned up in the course of my morphological work. In the case of the shoot, formative influences must act in the small apical region where we have the meristematic growing point with the primordia of the leaves. There is a presumption in favour of some sort of segmental construction of the meristem in relation to the leaves, whether this coincides with the cell-segmentation or not, and such a segmental construction is reflected in the vascular system. Can we in the first place distinguish any parts played by influences from the stem-apex and the developing leaves respectively? Unfortunately we know little or nothing of the anatomical relations in the rare cases of adventitious leaf-formation. We get a little insight into the respective influences of leaf and axis, however, when we compare shoots with well-developed leaves and those without leaves or with greatly reduced leaves; this may be done between distinct plants or between different regions of the

[13] An advantage that follows from this is that we get clear of the misleading metaphor of leaf-traces 'passing out,' &c.

same plant. It seems to emerge from such comparisons that, as regards the xylem at least, a central strand may be independent of influences from the leaves, while the latter may not only determine the leaf-traces connecting with the central strand, but may influence the periphery of this; the result is a cylinder of outer xylem continuous with the leaf-traces. This general conception is borne out by widely different plants, the correspondences between which are homologies of organisation. I may instance the stele of the Polytrichaceæ as analysed by Mr. and Mrs. Tansley, the stele of the rhizome and aerial shoots of the Psilotaceæ, of the Lycopods with larger or smaller leaves, and the stele of the ferns at various ages of the plant. The shoot of *Isoetes*, which is of the Lycopod type but has relatively large leaves, shows the composite nature of the stelar xylem particularly clearly and also suggests how the componen influences are at work in the meristematic region of the stem bringing about resultant structure.[14]

Owing to the small size of the shoot-apex it is difficult to induce deviations from the normal to show the respective parts played by the central axis and by the influences from the leaf-primordia. The reality of influences proceeding backwards from developing structures is better brought out when they may be present or absent, and for this lateral buds are of special interest. As a rule, the primary development of buds has proceeded far enough to determine the connecting vascular tracts, but in the case of the dormant axillary apices of *Botrychium* no influence has been exercised on the vascular structure of the main shoot. When, however, such a lateral apex is called into activity, it not only forms its own vascular system as it develops, but exerts an influence backwards through permanent tissue leading to the production of a 'branch-trace connecting with the adaxial face of the subtending leaf-trace. In *Helminthostachys* a similar connection is established with the stele of the main stem, and the influence may extend to the whole periphery of the main stele, inducing a continued or secondary production of xylem both behind and before the place of insertion of the branch.

These constructions were in a sense called forth by experimental interference, since they occurred in plants the normal apical growth of which had been arrested. Plants of *Osmunda* are normally unbranched and no indication of dormant lateral apices have ever been detected. I tried on young plants of *Osmunda regalis* the experiment of injuring or destroying the apex of the shoot, with the result that in a number of them branching was induced. The vascular relations exhibited considerable variety, but in some clear cases the branch was developed in an axillary position with regard to a leaf-primordium[15] and its vascular connection was with the adaxial face of the subtending trace in the same fashion as in *Botrychium* and in some species of *Zygopteris*. The disturbance of the normal growth had apparently brought out (in more or less irregular form) the system of relations governing the position of development of lateral branches. The result showed the correspondence with what is the normal condition in some Zygopterideæ. It has been said from the phyletic side, and on the whole rightly, that experiment cannot reconstruct history. In the light of Dr. Kidston and Professor Gwynne-Vaughan's conclusions as to the derivation of the Osmundaceæ from a Zygopterid ancestry this induced branching of *Osmunda* might almost be cited as a partial exception to the statement.

These examples will suffice to indicate the justification for a change of attitude in the study of the vascular system. Looked at in this light, the stele appears not as a characteristic thing inherited as such, but as a complex resultant. The problem gains in interest, new questions (which are different from though not antagonistic to phyletic problems) can be put as to stelar structure, leaf-trace structure, the venation of leaves, &c. We see this if we glance at the progression in stelar structure that accompanies the development of the young fern.

[14] This is confirmed by the origin of a similarly differentiated 'stele' in relation to the insertion of the successively produced roots at the base of the plant of *Isoetes*.

[15] In another case the leaf primordium appeared to be replaced by an axillary branch—*cf.* the case of *Plagiogyria* described by Bower, *Annals of Botany*, vol. xxiv. p. 434.

The phyletic explanation has been recapitulation. We have found reason to criticise the adequacy of this as applied to external form, and the same line of criticism applies to the stelar progression. In this also the early stages may be hurried over or absent and, still more significant, the early type of stelar structure may recur, when the shoot has fallen upon evil days and approximated in size of stem and leaf-form to the seedling condition. From such points of view the vascular system offers problems in general or causal morphology not merely of great interest but with some possibility of solution. Thus the parallel progressions from protostely to a medullated monostele, and from protostely to solenostely and dictyostely may be treated as problems in the expansion and condensation of a stelar structure, which is itself the resultant of a system of influences. Such parallel progressions are before us within the ferns and also in other groups of Vascular Cryptogams.

One of the most remarkable of these (which also affords an example of a change in anatomical construction related to a change in the external conditions) is seen in the occurrence of a solenostele in the rhizome of *Selaginella Lyalli*. This has been explained by the relation of the aerial branch-systems to the rhizome being similar to that of the leaf to the creeping fern-stem. Professor Bower's suggestion [16] that in some way the different construction of the rhizome is due to the horizontal position acting as a 'stimulating cause' seems more in accordance with the facts for this *Selaginella*. At any rate, Mr. Speakman, working in my laboratory, has found that in *Selaginella Lyalli* all the lateral branches of the rhizome become polystelic, but, while this is maintained in the erect shoots, those which grow into horizontal branches become solenostelic by a fusion of the ring of separate steles first developed. Bruchman also found that polystelic shoots laid horizontally on the soil grow on into solenostelic rhizomes. This is so fundamentally different from the relation of the solenostelic and dictyostelic condition in ferns as to suggest that the homoplastic resemblance is here probably not a homology of organisation but due to different factors.

There is ground for suspecting the anatomical method when it stands by itself and also for very critically considering explanations of structural change on the ground of utility. When in the attainment of any more complex whole (as, for instance, in the origin of the shoot, the ovule or the flower), a new system of relations is established and the external developmental morphology of the primordia and their mutual relations are changed, this will be reflected in the vascular system. The resulting change in the latter may, however, be profound and not appear as a gradual modification of the preceding vascular system.

In the numerous theories of the construction of the shoot the evidence relied on has been partly comparison of mature form, partly, though to a less extent, development, partly the vascular anatomy, and largely phyletic series, most of which are very questionable. I must touch on this subject, but do not propose to involve myself in the details of particular theories. They can be broadly divided into those which regard the stem as in one way or another built up of potentially and phyletically independent segments or phytons and those which regard the shoot as a phyletically pre-existing axis or stem from which the leaves have arisen by enation.

Considering the antagonism between these two lines of interpretation of the shoot that has held throughout the history of botany, I feel diffident in suggesting that there is much truth in both. In the light of general comparison the leaves cannot be looked on as mere enations from an unsegmented axis; nor, on the other hand, can the latter be regarded as composed of united leaf-bases. There seems an element of truth in the idea that the shoot can be analysed into segments composed partly of stem and partly of leaf; and another element of truth in the idea of a central column being clothed by some sort of pericaulome, though the form in which Celakovsky and Potonié have stated their theories and the evidence advanced makes their acceptance impossible. While the straightforwardness of the distinction between stem and leaf as put forward by Braun or in the strobilar theory is attractive and has apparent support in the apical development of the higher plants, it

[16] *Annals of Botany*, vol. xxv. p. 567.

seems impossible to overlook a segmental construction of the whole shoot in relation to the leaves.

The view of the general nature of the shoot to which we were led by a wide comparison including Algæ, Bryophyta, and Pteridophyta seems to enable us to understand in natural relation to one another the reality of the monopodial axis, its segmental construction, and the origin of the free portion of the leaves as outgrowths. The shoot is a new whole to be understood in the light of the method of elaboration of the single filament with subordinated appendages. As soon as the shoot is recognisable as an entity, the distinction can be drawn between these strictly subordinated appendages or 'leaves' and branches which repeat the whole construction. I cannot enter into the question of whether the fern-leaf is to be regarded as a transformed branch system, further than to state my opinion that the correspondence of the fern shoot as a whole with that of a Lycopod or a moss makes such a difference between their leaves unlikely.[17] The evidence in its support is drawn partly from an assumed origin of shoots from a hypothetical thalloid or pro-hepatic type and partly from vascular anatomy. The evidence on such questions from anatomy is, for reasons already given, perhaps the least satisfactory available, since the vascular tracts are established after the shoot as a whole has come into existence. They may to some extent reflect the original construction of the shoot, but may be largely independent of this.

If we test the opposed theories by their application to such a fern as *Ceratopteris* in the light of its embryology and apical development we find that the conception of leaf arising from leaf without the existence of a stem apex is not really supported by the facts. On the other hand, the segmentation of the shoot is evident throughout, owing to its coincidence with the apical segmentation. It is further most instructive to try to apply the various theories, whether phytonic or strobiloid, to the shoot of the moss with all its parallel correspondences to that of a higher plant.

Alternation of Generations.

The question of individual development led to the consideration of the morphology of the shoot. It also naturally leads us to glance again at the old problem of alternation of generations, for it is of profound interest to causal morphology that two very distinct individual forms should appear in the same life-cycle. The question thus raised concerns the nature of alternation, and is distinct from the historical question as to the origin of alternation, which for a time was regarded as constituting the whole problem. Any solution of the historical question depends on a knowledge of the lines of descent that have led to the various groups of the Algæ, Bryophyta, and Pteridophyta. We have seen how little prospect there is of this. The various examples of alternation seem best regarded as homologies of organisation, independent manifestations of a similar condition. They are not less but more valuable on that account in studying the nature of alternation. To find the common ground from which the correspondences in this respect in the life-histories of Algæ, Bryophyta, and Pteridophyta result we might have to go very far back, possibly to the life-cycle of unicellular organisms. Even there the phenomena might be of independent origin and the homologies not homogenies. Were this the case we should have to contemplate an intercalation theory of the vegetative tissues not only for the sporophyte but for the gametophyte. This is necessarily mere speculation, but it helps us to realise, as Dr. Scott once said, how insoluble the problem really is.

I wish to dwell for a little on an aspect of it which is more open to attack, however difficult. This is the analysis of the phenomenon as we see it in the life-history of the fern with the object of arriving at some idea of the causal factors in the difference between the two generations. In one sense the fern-plant and the prothallus appear like two individuals; in another the two stages are like parts of the same individual. We seem almost forced to assume that

[17] The cases in Algæ where a shoot appears to result from the subordination of branches to an axis are of great interest, but require further elucidation themselves, while the other line of elaboration is clear. The difficult cases are mainly found in the Fucaceæ.

the specific substance of a fern can, as it were, exist in two allotropic modifications, the properties of which are revealed in the unlikeness of the two generations. Presented thus, alternation of generations becomes a special aspect of the problem of individual development.

This aspect of the problem is, indeed, apparent within what we naturally regard as individual development, whenever this is discontinuous. Thus in the moss we see within the limits of the sexual generation a marked discontinuous development leading to the formation of the leafy shoots upon the protonema. The change to the more complex type of plant-body is in some way determined in a single cell, which proceeds to develop in a new fashion. In some cases the stimulus of light of sufficient intensity appears to be effective, but we know nothing of the internal factors at work. The change, however, is not rigidly pre-determined, and it is of interest to find it at first reversible; the apical cell of the young bud may continue as a protonemal filament, though this rarely occurs in an older shoot. As another instance of discontinuous development, where also we have hints of an explanation, I may remind you of the inflorescences of *Veronica Chamædrys*. They differ in a number of respects from the main vegetative shoot of the plant, for instance in having spiral instead of decussate leaves and a different type of hair. When Klebs,[18] experimenting on the transformation of reproductive to vegetative shoots, succeeded with inflorescence cuttings of *Veronica*, the unknown modification in the growing point resulted in a change to the whole alternative system of relations, the growth continuing as a vegetative shoot with decussate leaves. These two examples suggest that from the causal point of view the alternation of shoots and the alternation of bionts again become parts of the same problem. There is no sharp line between continuous and discontinuous development, but the discontinuity makes it easier to analyse, and perhaps experimentally attack, some problems of development.

This last consideration applies to the normal alternation of the prothallus and fern-plant; the new start from the fertilised egg gives an impression of abrupt discontinuity greater than that in the origin of the inflorescence of *Veronica* or the moss-bud on the protonema, but the cases are not essentially different. It is true that the egg at fertilisation appears as a separate little mass of substance in the venter of the archegonium, but after fertilisation there is the closest physiological connection between the prothallus and embryo. In a sense the latter behaves as if it were a special bud or branch of the prothallus.

Some years ago I attempted to re-state from the ontogenetic side the question of the different development of the enclosed egg-cell from the free spore. I assumed the two germ-cells to be 'essentially alike, the different products of their development depending on the different conditions.' The development of the zygote in relation to the enclosing gametophyte was regarded as the important factor. Though the position taken up was somewhat crude, it was useful in eliciting statements on the subject from a number of botanists; this response being more valuable than the stimulus. The view advanced has been criticised in most helpful fashion by Professor V. H. Blackman,[19] who took the ontogenetic ground to which I was endeavouring to shift the problem. He considers the egg and spore to be different, in that 'one has received from the plant which bore it a tendency to become a sporophyte, the other a tendency to become a gametophyte.' He further extends the idea of correlation, as explaining the orderly development of an individual, to the whole life-cycle, and considers 'the various stages as united together by a *cyclical correlation*, one stage influencing the development of the other.'

Professor Blackman's view does not seem inconsistent with mine, but together with it gives a better statement of the position. My attempt was really towards an explanation of this cyclical correlation. When germ-cells are separated from the parent body, as in the case of the various spores of a Uredineous fungus, any differences in their powers must have been impressed on them previously and are manifested given the proper conditions. So far as this goes, it applies to the spore of the fern and in part to the egg. But the latter is in a different

18 *Willkürliche Entwickelungsänderungen bei Pflanzen*, p. 69.
19 *New Phytologist*, vol. viii. p. 207.

case from the spore. While peculiarities may already have been impressed upon it, a metabolic relation with the prothallus still continues, and the developing embryo may be further influenced by the latter. The essential of the view I advanced was the possible importance of this continued influence on the retained egg and embryo as giving the clue to its different development as compared with the free spore. The idea of the retention of the egg may be made a little clearer by distinguishing between effective and ineffective retention. The mere fact that an egg develops within an oogonium or archegonium only amounts to ineffective retention if mutual relations are not established between it and the gametophyte. Thus retention is ineffective in *Vaucheria* or *Œdogonium*, but it is not clear that it is so in *Coleochæte*. As regards the egg in the archegonium, however, there is evidence of mutual relations between the embryo and the prothallus. We see changes in the calyptra and in other parts of the prothallus following on the presence of a developing embryo. On the other hand, the symmetry of the normal embryo and the position of its primary members is determined by the relation to the prothallus, and not by the external influences of light, gravity, &c.

In trying to form any idea of the nature of the influences which result in the cyclical correlation between gametophyte and sporophyte in the normal life-history the deviations from the normal met with in apogamy and apospory are of assistance; they have an importance for causal morphology which was almost wholly overlooked when the problem of alternation was stated from the phyletic point of view. The first examples of apogamy and apospory described in the ferns were associated respectively with an absence of the sexual organs or of sexual reproduction and imperfection or absence of the sporangia. Later work has not diminished the possible importance of this sexual arrest or soral arrest in some cases, but has shown that the problem is by no means simple or direct. To properly discuss it would require the consideration of examples, not only of the transitions between plant and prothallus, but of the mix-up of tissues and members of the two generations. Our knowledge of these has increased greatly, but in most cases we are only acquainted with the phenomena without being in a position to understand them. Let me remind you of some types of apogamy: apogamous development of an ovum; direct apogamy as a constant character of the particular species or variety; direct apogamy with the occurrence of structures intermediate between gametophyte and sporophyte; induced apogamy in potentially normal prothalli; the development of isolated leaves, roots, ramenta, or sporangia upon a potentially normal prothallus. On the other hand, we have as manifestations of apospory: apospory as a regular characteristic of certain varieties; aposporous development from the attached leaves of young plants; induced apospory from the primary leaves of normal ferns; induced apospory by the arrest of spore production; and the occurrence of intermediate growths between sporophyte and gametophyte. Clearly no one formula will cover all these cases. The nuclear facts we now possess are of great interest, but do not give an explanation of the phenomena. The whole subject is a most promising one for critical and thorough experimental work, all that has yet been done in this direction being of the nature of prospecting.

Let us now assume that the normal alternation is due to cyclical correlation and try to analyse this a little further in the light of the facts of apogamy and apospory. The simplest cases of direct apogamy show a fern sporophyte continuous with the cushion of the prothallus, but with the relative position of its members the same as that of a plant developed from a fertilised egg. We are naturally inclined to think first of a change to the alternative system of relations and to remember that in the case of the normal embryo this takes place in a similar relation to the prothallus. When, however, we consider the strange cases of the perfect development of isolated members or tissues of the sporophyte on or in the prothallus, we seem forced to think further of special formative influences that are of the nature of substance rather than of a system of relations. We are confirmed in this by the fact that a number of cells may be simultaneously and collectively influenced, and that the influence may be reversed. This holds both for apogamy and apospory; in the latter when the prothalli develop upon leaves attached to the plant, it is difficult to see

how either external or internal influences equivalent to the usual relations can come into play. There is no nuclear change, at least of the nature of meiosis, and there seems nothing for it but to assume some material modification involving the change to the alternative condition of the specific substance expressed in the prothallus. The interest of these considerations, tentative as they must be, lies in the way in which they associate two explanations of development, the influence of particular, unknown determining substances and the system of relations.

The whole question is fortunately not complicated with adaptation or any gradual origin of these deviations from the normal, and affords a particularly clear example of a problem in causal morphology. The perfect development of isolated members without the usual relation to the rest of the plant-body has an important regulative bearing on the current assumption that every stage in development is determined by the preceding stage. In this connection a most interesting parallel can be traced between the appearance of perfectly formed roots, ramenta, sporangia, and vascular tissue in induced apogamy and the development of bones, teeth, and hairs in dermoid cysts occurring in the human ovary or testis. The further study of the conditions of development in such abnormal cases may do much to enlighten us as to the factors concerned in the normal ontogeny.

The Seed and its Embryo.

So far the problems considered in illustration of the possibilities of a general causal morphology have been suggested by the fern. The nature of roots, of the sexual organs, and of sporangia might also be profitably looked into from the non-phyletic point of view. I shall instead step beyond the fern and glance very briefly at some problems of the seed and the embryo of the seed plants. The story of the great additions made of recent years to our knowledge of Palæozoic seeds is familiar to us all. It remains an open question, however, to what extent seed-plants are to be regarded as of polyphyletic origin and in particular to what extent their seeds are homogenous or are homologies of organisation. In spite of the wonderful widening of the field of comparison by the discovery and investigation of the Pteridosperms we have no compelling evidence of actual lines of descent showing steps in the origin of the seed. The presumption is now in favour of an origin of all or most seed-plants from ancient Filicales, the earlier view of their derivation from the Lycopodiales being weakened or abandoned. Since this phyletic problem lies, in part at least, within the period of geological history, direct evidence may be hoped for. But in the light of the progress of opinion on other large questions of descent we must remember the possibility of parallel evolutions and may even suspend judgment as to whether the Cycadophyta have been derived from Pteridosperms or the Angiosperms from the Bennettitales. Comparisons of the organisation of the whole plant seem somewhat forced in both cases.

Looking at the facts broadly, we can hardly escape from a very strong suggestion of parallel development, affecting a number of distinct groups. There is evidence of this in the case of heterospory, which is found at various points in the Lycopodiales, Filicales, and Equisetales, usually with no indication of its being a gradually acquired or an adaptive character. Heterospory is otherwise a darker problem than the origin of the seed-habit to which it was presumably the preface. The study of the variety in early seeds has suggested to experienced investigators that seeds also may be the result of independent development.[20]

From the point of view of causal morphology the seed appears to present problems parallel to those of the enclosure of the archegoniate sporophyte in the archegonium. The distinction made between effective and ineffective retention applies here also. The fact that the megaspores of some species of *Selaginella* germinate within the sporangium is not really an approach to the seed-habit; it is ineffective retention so far as results on either the spore-contents or the sporangium are concerned. In the development of the ovule, however, we find the embryo-sac constituting with the investing tissues a new

[20] *Cf.* Oliver and Salisbury, *Annals of Botany*, vol. xxv. p. 46.

whole. There is the same likelihood of effective correlations as in other portions of the individual plant in which a new relation is set up. Cases in which a new influence affects the meristematic structure of the plant—as, for instance, in galls or the root-tubercles of the Leguminosæ—seem to be parallel. They exhibit a differentiation of the investing tissue in relation to the enclosed portion and a suggestive similarity between the vascular arrangements in the root-tubercles and those in the more bulky ovules. The new construction in the galls cannot be regarded as in any sense adaptive or purposive on the part of the plant. We are forced to look at it causally, and perhaps such a mode of regarding the ovule and seed may prove to be the most helpful. On such a view the ovule would be a megaspore gall, later containing an embryo plant, and the problem would concern the system of relations existing between the parts as development proceeds. Corresponding causes might independently result in corresponding constructions and the homologies between various seeds or the contents of various embryo-sacs, though real, be homologies of organisation. This is consistent with the ideas we were led to entertain in the case of the shoot and of alternation of generations.

As a last example the embryo of the seed-plants may be referred to, though I must not venture far into the facts. While exhibiting differences, the embryos of various seed-plants present certain common features which contrast with the embryos of all spore plants, even the heterosporous forms. How far are these peculiarities causally connected with the seed-habit and to what extent are they marks of phyletic unity? We have seen grounds for suspecting that seeds are parallel developments. What follows as to the construction of the embryo if we contemplate such independent origins from ancient Filicales? The suspensor has already been considered, and we can regard it as a persistent construction finding a use in some cases. In the embryos of the Filicales we see a very clearly marked type with a single relatively large cotyledon and a hypocotyledonary region between the shoot and root. Without entering into the question of whether or not the Monocotyledons were derived from Dicotyledons, it seems clear on broad phyletic grounds that a single cotyledon condition lies behind the dicotyledonous or polycotyledonous condition. There is thus a presumption that there has been a change in the primary members of the embryo in this direction. It seems worth while to emphasise that, from a phyletic point of view, there is a real inconsistency between the origin of seed-plants from the Filicales and the relative primitiveness of dicotyly. Is it not possible that the conditions of early embryonic growth in the more or less cylindrical seed may have led to a more symmetrical construction of the embryo, and that the dicotyledonous or polycotyledonous condition, on the one hand, and the monocotyledonous condition with an apparently terminal cotyledon on the other, are two alternative expressions of this? [21] For reasons already given, the anatomy would follow the morphological change, and would have to be considered in the light of this, and not as affording safe evidence by itself.

Conclusion.

I have touched on a number of large questions, any one of which demanded separate treatment. My concern has not, however, been with them individually but as cognate problems justifying the deliberate adoption of a causal explanation as the aim of morphological work. I have confined myself to problems bearing on the development and self-construction of the individual and tried to treat them so as to illustrate the causal attitude and possible lines of attack. Preliminary speculations on the questions considered can at best contain a germ of truth, and must be subsequently adjusted in the light of further facts. I have discussed these questions rather than the smaller modifications in individual development shown in metamorphosis, partly because the latter have of late years been treated from a causal point of view [22] and

[21] Recent observations of Coulter and Land on the embryo of *Agapanthus* appear to afford direct support to this view arrived at by quite a different line of comparison.—*Bot. Gaz.* vol. lvii. p. 509.

[22] *Cf.* especially, 'The Fundamental Problems of Present-day Plant Morphology,' *Science*, N.S. vol. xxii. p. 33, and other works by Goebel,

partly because I wished to consider questions that immediately affect us as working morphologists.

Did time allow, we should naturally be led to recognise the same change of attitude in biological science toward the problems of the origin of new forms. Questions of bud-variation and mutation are clearly akin to some of those considered, and the whole subject of genetics is a special attempt at a causal explanation of form and structure and the resulting functions. Close co-operation between the morphological analysis of the plant and the genetic analysis attained by the study of hybridisation is most desirable. It is especially desirable that both should deal with structure as well as with form, and in the light of individual development.

The causal factors which have determined and guided evolution can be naturally regarded as an extension of the same line of inquiry. The Darwinian theory, and especially the exposition of the principle of natural selection, was the greatest contribution ever made to the causal explanation of the organic world. Strangely enough, it led to a period of morphological work in which the causal aim was almost lost sight of. Why evolution has taken place in certain directions and not in others is a problem to the solution of which causal morphology will contribute. The probability of orthogenesis both in the animal and vegetable kingdoms is again coming into prominence, however it is to be explained. When we consider the renewed activity in this field it is well to remember that, just as is the case with causal morphological work, we are picking up a broken thread in the botanical web. Lastly, as if summing up all our difficulties in one, we have the problem of adaptation. In attacking it we must realise that use and purpose have often been assumed rather than proved. We may look to scientific ecological work to help us to estimate the usefulness or the selection value of various characters of the plant. On the other hand, causal morphology may throw light on whether the 'adaptation' has not, in some cases at least, arisen before there was a 'use' for it. The hopeful sign in the recent study of these greater morphological problems is that the difficulties are being more intensely realised, and that rapid solutions are justly suspect. The more the causal attitude is adopted in ordinary morphological work, the more hope there is of these larger questions being inductively studied rather than argued about.

The causal aim is essentially different from the historical one, but there is no opposition between causal and phyletic morphology. They are rather mutually helpful, for there has been an evolution not of mature plants, but of specific substances exhibiting development. A deeper insight into the nature of ontogeny is thus bound to be of assistance to phyletic morphology, while the tested results of phyletic work afford most valuable guidance in general causal morphology, though this cannot accept any limitation to single lines of descent in its comparisons.

I have tried to bring before you the possibilities of causal morphology partly because the same attention has never been given to it in this country [23] as to other branches of botany and partly because if morphology be conceived in this broader spirit it need not be said that it has no practical bearing. I should not regard it as a serious disability were the study of purely scientific interest only, but this is not the case. When, if ever, we penetrate into the secrets of organisation so far as to be able to modify the organism at will (and genetics has advanced in this direction), the practical possibilities become incalculable.

Probably all of us have reflected on what changes the war may bring to botanical work. It is impossible to forecast this, but I should like to emphasise what my predecessor said in his Address last year as to pure science being the root from which applied science must spring. Though results may seem far off, we must not slacken, but redouble our efforts towards the solution of the fundamental problems of the organism. This can be done without any antagonism between pure and applied botany; indeed, there is every advantage

[23] One of the few exceptions to this is the excellent semi-popular lecture delivered to this section at Southport by Professor Farmer. ('On Stimulus and Mechanism as Factors in Organisation.' *New Phytologist*, vol. ii. p. 193.) Also Address to Section K at the Leicester Meeting (1907).

in conducting investigations on plants of economic importance. It would be well if every botanist made himself really familiar with some limited portion of applied botany, so as to be able to give useful assistance and advice at need. The stimulus to investigation would amply repay the time required. Even in continuing to devote ourselves to pure botany we cannot afford to waste time and energy in purposeless work. It is written in 'Alice in Wonderland' that 'no wise fish goes anywhere without a porpoise,' and this might hang as a text in every research laboratory.

A plant is a very mysterious and wonderful thing, and our business as botanists is to try to understand and explain it as a whole and to avoid being bound by any conventional views of the moment. We have to think of the plant as at once a physico-chemical mechanism and as a living being; to avoid either treating it as something essentially different from non-living matter or forcibly explaining it by the physics and chemistry of to-day. It is an advantage of the study of causal morphology that it requires us to keep the line between these two crudities, a line that may some day lead us to a causal explanation of the developing plant and the beginnings of a single science of botany.

British Association for the Advancement of Science.

SECTION L: MANCHESTER, 1915.

ADDRESS

TO THE

EDUCATIONAL SCIENCE SECTION

BY

MRS. HENRY SIDGWICK,

PRESIDENT OF THE SECTION.

WHEN I look at the names of many of my predecessors in this Presidential chair, when I read their addresses, or when I consider what the work of the Section ought to be, I feel that an apology is needed for my being here at all.

Let me say at once, however, that it is not because of my being a woman that I feel this. It is true that I am the first woman who has had the honour of presiding over Section L. But it is obviously very fitting that a woman should sometimes do so; and this not only because women are as much concerned with the results of Educational Science as men are—that might be said about all departments of science; nor only because the material on which education works—the human material to be educated—is approximately evenly divided between the sexes. A more important consideration is that women have the largest share in the work of education. This is clear if we take education in its widest and fullest sense, and include in it what is done in the home as well as in the school, beginning as it must with the earliest infancy. But it is also true if we limit the meaning of the word education—in the way that is constantly done, and is I think usually done in the discussions that take place in this Section—to that part of it with which the professional educator, the school or college teacher, is concerned. For the fact that the school teaching, not only of girls but of the younger children of both sexes, is mainly in the hands of women, results of necessity in there being a larger number of professional teachers among women than among men.

May it not be added that in some departments of education women have appeared to take their profession more seriously than men so far as this can be measured by the trouble taken in training for it? For I think I am right in saying that among persons proposing to teach in secondary schools more women in proportion than men have hitherto availed themselves of opportunities for professional training.

From another point of view, too, the education of women and girls has an interest which, though not different in kind, is greater in degree than that of the other sex. I mean in the rapidity of its growth and development since the middle of the last century. The development of school and university education and of technical education has, of course, been very great for both sexes. Much attention has been devoted to improving its quality and perhaps even more to increasing its quantity by making it more accessible to all classes of people. But in the case of girls and women the progress has been greater and more remarkable than in that of boys, for it started from a lower level, and notwithstanding this it would, I think, be difficult to point out in what respects the educational opportunities of women are now inferior to those of men. I say this, of course, in a general sense, and without prejudice as to controversial questions of detail such as the merits of the methods and curricula deliberately adopted for different schools.

The Report of the Schools Inquiry Commission published in 1868, in what it says about girls' education at that time, gives us a standard of comparison

and a means of estimating the progress made. It has often been quoted, but may bear quoting again. The Commissioners say: [1]

'The general deficiency in girls' education is stated with the utmost confidence, and with entire agreement, with whatever difference of words, by many witnesses of authority. Want of thoroughness and foundation; want of system; slovenliness and showy superficiality; inattention to rudiments; undue time given to accomplishments, and those not taught intelligently or in any scientific manner; want of organisation—these may sufficiently indicate the character of the complaints we have received, in their most general aspect. It is needless to observe that the same complaints apply to a great extent to boys' education. But on the whole the evidence is clear that, not as they might be, but as they are, the girls' schools are inferior in this view to the boys' schools.'

This was what could be said of schools in 1868, and is certainly in striking contrast to what could be said now. And if we turn from the schools to higher education we find this was practically non-existent for women at that time. Its absence was indeed one cause of the badness of the schools. The schools were bad because the teachers were inadequately educated. 'The two capital defects of the teachers of girls,' as one of the Assistant Commissioners (Mr. Bryce, now Lord Bryce) reported, 'are these: they have not themselves been taught and they do not know how to teach.' These defects were, of course, partly due to the badness of the schools, and the want of any standard enabling the general public and the teachers themselves to judge of their badness. So far it was a vicious circle. The teachers were badly taught in bad schools and handed on the bad results to the schools they later taught in. But the defects were partly due to the absence of opportunity for them to carry their own education beyond that of their elder pupils—to obtain that higher education which men obtained at the Universities. This was pointed out by the Commissioners, and their Report acted as a great help and encouragement to those who had already realised the need of higher education for women, and gave an important stimulus to the foundation of Colleges for Women first at Cambridge and then at Oxford.

The Commissioners' Report also greatly encouraged the movement already in progress for the improvement of girls' schools—the movement in which Miss Buss, of the North London Collegiate School, and Miss Beale, of the Cheltenham Ladies' College, were among the pioneers, and in which the opening of Local Examinations to Girls in 1865 by Cambridge was an important step. The cautious and anxious way in which the Commissioners refer to the possible effects on girls of more exacting school work and of examinations is amusing to read now. But the Report of the Commission helped in the progress of girls' education in still another way, for it was instrumental in securing the recovery for the secondary education of girls of endowments which had been allowed to lapse into the service of primary education or to be absorbed by boys; and the division between girls and boys of some endowments not specifically assigned to either sex by the founders. Twenty years ago—in 1895—the Charity Commissioners in their Annual Report gave striking testimony to what has been done both in this way and by new endowments:

'There is reason to think,' they said, 'that the latter half of the nineteenth century will stand second in respect of the greatness and variety of the charities created within its duration to no other half-century since the Reformation. And, as to one particular branch of Educational Endowment, namely, that for the advancement of Secondary and Superior Education of Girls and Women, it may be anticipated that future generations will look back to the period immediately following upon the Schools Inquiry Commission and the consequent passing of the Endowed Schools Acts, as making an epoch in the creation and application of endowments for that branch of education similar to that which is marked, for the education of Boys and Men, by the Reformation.'

And the flow of endowments for this branch of education has not ceased since the Report just quoted from was written. As examples of it I may remind you of the St. Paul's Girls' School, the extension and rebuilding of

[1] *Report of the Schools Inquiry Commission*, p. 548.

Bedford College, University of London, and the large sums given for the domestic department of King's College for Women.

Though, however, as the Charity Commissioners say, a great impulse was given to girls' education by the Report of the Schools Inquiry Commission and the legislation as regards endowments that followed, I think that, even without these, great progress would have been made, though probably less rapidly. The desire for it was already there. Women who had themselves suffered from the previous deficiency were working for improvement, and sympathetic men friends were helping. It was becoming more and more obvious not only that women teachers must have adequate opportunities of learning, but that the home no longer in itself afforded sufficient scope for the energies of the daughters, especially unmarried daughters, of the professional classes, and that they must be trained for other useful work. The supply of suitable education followed the demand, as generally happens when the demand is strong and clear. The very mention by the Charity Commissioners in the passage I have quoted of the *creation* as well as of the *application* of endowments for the purposes of female education is evidence of the active public interest in the matter. The spirit which has led during the last half-century to the liberal endowment of education for girls and women from private sources has also led the State, and public bodies generally, to consider girls equally with boys in all public administration of education or of educational funds. The same spirit has led the newer universities without exception to admit women to their benefits on equal terms with men. And at the same time the creation of some professions and skilled industries—*e.g.*, sick nursing—by women, and the opening to them of others, together with the general movement in favour of professional training for professional work, have led to the great development of opportunities of technical or vocational training for women as well as for men.

This immense—almost revolutionary—change, as regards Educational opportunities for women, which has occurred within the recollection of people of my age, and which must be attributed largely to the efforts of women themselves, is, I think, very striking; and it certainly, as I said, fully justifies the selection of a woman to preside over the Educational Section of the British Association. The apology I feel to be needed is for the particular woman selected. For it is the Science of Education, or at any rate the Science and Art of Education, that this Section presumably exists to advance, and I am no educator, no teacher, I have made no psychological study of young people from an educational point of view, nor of the different methods of teaching suited to different ages, no statistical investigation of the influence or particular curricula in training the mind or furnishing it with useful information. I have, in short, neither made contributions to the science of education nor practised the art. Any work I have done has been on the administrative side, and I can speak only as a member of the general public—not as an expert. And what is there new, in a subject so much discussed, for a member of the general public to say? An illuminating address is, I fear, under the circumstances impossible.

Not that I regard the view of the general public as unimportant. Indeed, I am not sure that a good case could not be made out for having a mere member of the general public as such as president from time to time. The general public must, as all will admit, decide what is to be spent on education, or, more strictly, on schools and colleges and professional educators, out of both public and private income—it is for them to decide on its relation to other social and family needs. But the concern of the public with education is not merely financial and administrative. It is more intimate than that. For education is not a subject like physics or chemistry on which only an expert has a right to an independent view. There are, no doubt, aspects of it of which only the expert can properly judge, there are experiments in it which only the expert can advantageously try, and there are, of course, departments of it in which the opinion of the expert is indispensable. But without depreciating either the science or art of education, it is clear that when we take education in its widest sense it concerns everybody, and almost everybody is bound to have views about it. Each generation as a whole is responsible for handing on to the next the control over matter and mind, and the power of co-operation, which it has itself inherited from its forbears and added to, and which it must put its successors in a position to add to further. It is on this

that the progress of the human race depends; without it each generation would have to start fresh from the beginning, and we should still be in the position of primitive man.

But the larger and more important part of education in this wide sense is done first in the nursery, and then, as the child gets beyond babyhood, by means of its own observation and imitation of its elders; while much is done by experience gained in mixing with others of its own age, and much by the exercise of responsibility. The education thus obtained, combined with precepts and with tales handed down orally, sufficed for our ancestors until the increasing complexity of life made it important for the rising generation to acquire skill and knowledge which mere imitation and experience could not give. When this happened division of labour took place in this as in other departments of life, and led to the introduction of the professional educator—that is, the educational expert who has the art of imparting the needed knowledge and skill, or at least of shortening the process of acquiring them. We may observe that his services are now required by all and not, as was once the case, only by those preparing for the learned professions. This work of the professional educator is what our Section of the British Association is mainly concerned with, and the methods to be employed are best judged by the professional educators themselves. But the co-ordination of their work with the whole process of education, its place in the production of good citizens, must, as I have said, be judged, not by the professional educators alone, but by the whole body of the nation. The general public must not only be regarded as capable of exercising judgment on educational matters, but should be encouraged to feel that it is its duty to do so.

If we judge by the amount of talk which goes on about education, it would perhaps seem that the public is fully aware of its responsibilities. And yet I think there are indications that in some respects it fails to grasp them, and is disposed to depend too much on the professional educator; allowing itself to be confused by our habit of using the same word 'Education' in both the wider sense, of which we have been speaking, and also in the narrower sense of book-learning. The sense of proportion seems to me to be sometimes seriously lost from this cause.

I was impressed with an example of this exhibited a little while ago in a correspondence in the *Times* about the employment of the older boys in the elementary schools of country districts to do some of the work on the farms in place of farm-hands who have enlisted. One group of the correspondents, looking at the question from the point of view of agriculture, thought the advantage derived by the boy from his last year of school training was of small value to the country compared with the work he could do on the farm. The other group, looking at the question from the point of view of the school, thought it monstrous that what they called the 'education' of the boy should be in any way curtailed. I am not at the moment concerned with the controversy itself, nor am I taking the side of either group of disputants. There is, of course, much to be said on both sides, and the decision should probably vary with the locality, and the work, and the farmer, and the boy. But what struck me was that all the disputants seemed to regard education as beginning and ending at school. None appeared to think of it in its wider sense. None referred to the great effect it might have on the boy's future life and character to feel that in a grave national crisis he had 'done his bit'—an effect which would perhaps be all the greater if he felt he was sacrificing something to make up for which special effort might be needed later. I have seen the view of the gain to boys and girls from helping in the emergency put forward since, but not in the particular newspaper controversy in question, nor, I think, in connection with the loss of a year of schooling.

And there was another aspect of the question which did not seem to excite attention. I mean the possibly bad educational effect, in the wide sense, of preventing the boy from doing the work. To keep him at school, if he was conscious that his services were needed elsewhere, could not but tend to concentrate his attention on himself and the importance of his own schooling, and could not but tend to produce to some extent the deplorable temper of mind which leads some young people, a little older than the schoolboys over whom the controversy raged, to regard self-development as the aim and object of

existence. This is certainly not the attitude of a good citizen—and to produce good citizens should, as we probably all agree, be the principal aim of education. The particular difficulty to which I have referred seems inseparable from compulsory education, and probably cannot be altogether got over. The thoughtful girl of twelve, not absorbed in herself, must sometimes wonder whether her school-work is really as valuable as the help she could give her mother in some special difficulty or strain, except on the assumption that her own development ranks above all other objects.

Of course, the higher the relative value we put on scholastic education the less important will the loss of other educational influences appear to us. And perhaps at this point I had better frankly confess—what is, I fear, another defect in my qualifications as President of the Educational Section—namely, that I am not an enthusiast about education in the same sense that most of my hearers probably are. I read the other day in a review of the life of an American educationist that—

'He was penetrated with two characteristics which are the saving clause of the American and every other democracy, a reverence for learning and a flaming belief in education as the condition of success in any scheme of popular self-government.'

In the reverence for learning I am with him, but I could not describe my belief in education—education, that is, in the sense here meant, namely, school and college education—as 'flaming.' I cannot, for instance, believe, as some seem to do, that by keeping children a year longer at school we should regenerate mankind, or at least secure as a matter of course great improvement. Why, you may ask, if I am not an enthusiastic believer in education, have I spent so much of my life—my time, my energy, my means—in helping to provide opportunities of University education for women? The answer is that I do believe very much in giving as many people as possible educational opportunities—meaning by that in the first place the means of preparing for their work in life. Those who are going to teach, for instance, must obviously learn first, and, as I have just reminded you, women's opportunity of doing this was lamentably deficient half a century ago.

But secondly—and this is not at all less important—I mean by educational opportunity the means of satisfying intellectual curiosity, every spark of which should be fostered. For it is to intellectual curiosity that progress in knowledge, including physical science, is mainly due. And intellectual curiosity is an important adjunct to the mental processes involved in understanding the world we live in, a valuable aid in the formation of a good judgment, and a great assistance in practical life. Intellectual curiosity and æsthetic sensibility are, moreover, the mainsprings of culture—that is, of some of the highest pleasures we can enjoy.

You will doubtless agree with this, and will agree, further, that without intellectual curiosity no amount of accumulated information can be properly assimilated, or will produce either culture or knowledge of permanent value. In its absence the pupil may pass through school and college with little advantage apart from discipline, beyond the acquisition of elementary skill in reading, writing, and arithmetic, and if he has a good memory a barren knowledge of some facts. You will probably add that it is one of the most important functions of the teacher to endeavour to produce this intellectual curiosity when absent or in abeyance, and that the zeal of the professional educator in this direction is a strong reason for enthusiastic belief in school education. It would be, I grant, if we could hope that the teacher's success would always be equal to his zeal; but notoriously this is far from being the case, and the failure is by no means always due to want of intelligence in the pupil any more than it is due to want of capacity in the teacher. In many cases, in all classes of society, the spark of intellectual curiosity—the response in the pupil's mind to educational stimulus—cannot be fanned into flame through book-learning alone, and yet may be there all the time ready to burst forth when it comes into contact with the needs of actual life and work. It may even be there, and fail to respond to imposed lessons, while it would blaze up if the pupil could by any means be induced to desire to learn before he is taught. It is partly because it is so important, if and when the desire to learn comes, that the boy or girl,

man or woman, should be armed with the instruments which may give them independent means of acquiring the knowledge they desire, so far as this can be acquired through books, that we compel parents to send their children to school. No doubt, however, an even more important reason is our now almost universal use of reading and writing as a means of communicating with each other. The more widespread these arts are, the harder it is for anyone who has not acquired them to keep abreast of his fellows. But even now it would, of course, not be impossible, and the use of such phrases as compulsory education, in which education merely means the reverse of illiteracy, tends, I think, in itself to obscure the apprehension of what education really is and to reduce the general sense of responsibility for it, and particularly that of parents.

Many years ago, before the days of compulsory education, or at least before it had time to produce any effect, I knew a man in the South of England who had had no school education, or practically none. I believe he could read a little with effort, but he could neither write nor keep accounts, so I was told. His wife did these things for him when they were necessary. He was, however, a good farmer, farmed a considerable amount of land, and acted as manager or agent under the landlord for a large estate. He knew his business thoroughly, had the power of managing men, and was much respected. It is impossible not to regard such a man as a more valuable member of the community, and a better-educated man in some respects, than many of those who climb the educational ladder to become clerks in an office. But, of course, such a man must have regretted that he had not had opportunities of schooling in his early youth—that he had not acquired the art of writing while he still had leisure. The want of the three R's must have been a serious handicap, only overcome by unusual ability. And, in fact, no one now doubts that it is almost as important to acquire these elementary arts as to learn to speak or walk. It is with the question of carrying school education further that doubt arises whether it is really the best education for everybody, and whether we ought to regard the person whose scholastic education has been longest, or who has succeeded best in examinations, as therefore necessarily the best educated.

I do not mean in saying this to set the practical man above the man of learning. Of course we want both, and we should like our schools to help to develop both. The value to the world of good scientific and literary work is enormous. And so far as science is concerned the British Association exists to bring home to the general public its value and interest, and consequently the importance of men who can advance it. Nor do I mean in what I have said to suggest any divorce between practice and learning. The business of most of us is practical, but what is to be desired is that everyone capable of it should combine practical ability—whether in manual work, or in organisation or administration, or in any other line—with a desire to learn; and that not only in relation to his work in life, but in a wider sphere. And, of course, we must wish that the means to satisfy this desire should be within everyone's reach. My point, therefore, is not that learning is not valuable, but that it is of little value unless it meets a desire in the learner's mind. And here the parents come in. The required attitude of mind is much more likely to be inspired by parents who possess it, than it is by the school. Or let us say that those children are most likely to grow up with it whose parents combine with the school to stimulate it. Unfortunately the result of compulsory primary education has not been to promote any sense of responsibility in parents as regards this; at least that is my belief. And I may, I think, appeal to Scottish experience in support of it.

The institution of parish schools is, as is well known, older in Scotland than in England. They date there from the Reformation, and were part of the ecclesiastical organisation initiated by John Knox. In the scheme drawn up by him and his colleagues education had a great place. The parish schools, in which Biblical instruction was foremost, were put in charge of the Church and long needed its efforts for their maintenance. Starting in this way the zeal for school education had become traditional. All respectable parents aimed at giving their children the best education they could. There was a strongly rooted sense of duty in the matter, and this from a double motive. They sent their children to school not only to help them to get on in the world, but because of the traditional association of knowledge and religion. Observe

the educational value of this second motive. I am not looking at it from the religious point of view—that is not my business to-day. But as an instrument of culture the value of a desire for learning, based on something other than its relation to worldly success, is obviously great. It may be that the school education actually prevailing in Scotland is better now than that of fifty years ago, that the examination of the school inspector is more searching if less stimulating than was that of the Presbytery, that the average or backward child is less sacrificed to the clever one than used to be the case, and that general intelligence is more developed. But the parents, who felt their children's schooling to be their private concern, valued it more, took more personal interest in it, and felt more personal responsibility for their children's progress than parents can do now. And it is a serious question whether the loss of this close link with home life has not had a bad educational effect, taking education in its wider sense, which is not compensated for by possible improvement in the schools.

I must admit that in saying this I have in mind only a limited area. I have made no wider investigation. The population I am thinking of is an entirely rural one in a purely agricultural district in the South of Scotland, with which I was intimately acquainted as a young woman, and which I revisit from time to time. In such a district compulsion to send the children to school was unnecessary. It probably was required in the large towns and the more industrial parts of the country. I do not complain of the introduction of compulsion, but it did strike me at the time of its introduction that it was of very doubtful advantage in my own part of the country; and this impression has not diminished since

To see if it was shared by others I wrote to a friend, more familiar with the district than I am now, to ask whether he did not think that parental interest in the children's school education had decreased, and also whether he thought that, as judged, for instance, by the books they borrowed from the parish library, the grown-up population was less inclined to serious reading than they used to be. I received from him a very interesting reply. He agreed with what I have just said as regards the first question, and after speaking of the warm and genuine wish in old times to give the children a good education, added :

'The parents might, indeed, let their older children be absent for short times from school for light farm work or the like. But this was more than made up for by the zeal with which they were sent to winter evening classes which could be gathered then far more easily than now. It is an unfortunate effect of legislation that it has largely deprived us of the great asset we had in the keenness of parental interest. It came about in this way. Government made it compulsory that no child should be employed in wage-earning who had not passed the fifth standard. Almost instantly the ideal of our people was lowered. A child was "educated" who had passed the fifth standard! And when by and by Government made it compulsory that a child should be at school till fourteen years of age, the parents in many cases felt this hard upon them, and our School Board every year has applications for permission to children to work before they are fourteen on various pretexts. I do not say that our people are not interested in their children's education. They still inherit that interest But *compulsion*, and the fact of the responsibility being taken by Government, has greatly changed their attitude.'

With regard to my second question—'Whether there is in country parishes as much reading of serious books, books of weight, history, travels, &c.'—he says he 'must answer *No.*' He thinks that the young people are perhaps more intelligent than they used to be, 'but the reading is in enormous proportion novels and very light literature.' He goes on to tell me of an old man who died two years ago 'of the finest old Scottish type—devout, independent, interested in religious reading, in lives of men like Livingstone, in travels (he was reading Nansen in his ninetieth year and most interested in his nearing the Pole). But the list of books in his steady reading from the library here was of quite different character from that opposite other names in our catalogue of the same rank.' He says also that forty or fifty years ago good audiences could be got for lectures—historical, travel, &c., but that now a good

audience can only be got for concerts, entertainments, or at most lectures with lantern pictures. All this seems, as far as it goes, to show a diminution in culture, incapacity for the higher intellectual pleasures, in fruitful curiosity. My correspondent is not prepared, however, to say that this change is due to changes in school education. It comes, he thinks, 'of the different spirit in young people, less under authority, indulging more in pleasures, not pressing hard or thinking they need this in order to get on' He thinks, in short, that the young men now are more self-indulgent and less energetic than they were, and he looks to the nobler spirit which the War has called out to carry us into better ideals of life. He may be right in thinking that causes independent of school education have produced the result. But we must admit that if it is true that, concurrently with a school education improved in some important ways, there has been a diminution in intellectual interests—in culture, in short—the school education has at any rate failed in one of the objects aimed at.

Well, you must take these views about a particular country district for what they are worth. Facts observed among a comparatively small number of people may not represent the average. Moreover, my correspondent and I are both old—we could not remember, or think we remembered, the state of things fifty years ago if we were not—and you may, if you think proper, discount what we have to say, on the almost proverbial ground that old people put the Golden Age behind them. I am not, however, myself conscious of any such tendency. I believe very much in progress, and look forward to a gradually improving world, and I believe we are on the whole improving in educational ideals and educational methods as in other things. But it behoves us to watch what we do, and not to acquiesce, if we can possibly help it, in loss on one side without being very sure that it is more than compensated for by gain on the other. The loss of the parents' real co-operation where it has existed, and the failure to gain it where it has previously been absent, is serious. It is serious even if it is limited to the intellectual side of education and does not extend to the formation of character, as I fear it sometimes does. With the greatest zeal the schoolmaster cannot replace the parents, nor even the parents' influence in producing the right attitude of mind in the pupil. And it is at the very least doubtful whether the better teaching which improved methods secure to the pupil can make up for any loss of spontaneous desire to put his own mind into the effort of learning for learning's sake.

And so I come back to the point that the general public must be encouraged to take its share even in the part of education carried on at school and college, and in particular those members of the general public who are parents of pupils. But this conclusion is rather barren, for I have no very definite plan to suggest for carrying it out. The State cannot now, even if it would, abandon the responsibility for the elementary school education of the children, and even if it could it is more than doubtful whether it would be desirable. For though we have now secured that all parents shall themselves have had school education, we still cannot trust them all voluntarily to give that advantage to their children. So the drawback must be put up with that parents cannot feel the same degree of responsibility resting on themselves when the responsibility is undertaken by the State.

It is to be hoped, however, that we shall be very careful how far we entrust to the State the regulation of education higher than the primary. Bureaucratic regulation may be well adapted to produce German *Kultur*, but it is not the way to secure the attitude of mind which leads to freedom, independence of thought, and culture in the best sense. And it is very apt to lead to want of independence in the teacher.

Probably our best hope for progress in the right direction lies in movements like the Workers' Educational Association, where we have voluntary effort put forward to satisfy spontaneous desire to learn. As this movement extends we may hope more and more to get a generation of parents who, having themselves experienced intellectual curiosity and the joy of satisfying it, who, having themselves felt the gain of a wider outlook on men and things, may by their example inspire their children with a similar disinterested desire for learning and culture.

British Association for the Advancement of Science.

Section M: MANCHESTER, 1915.

ADDRESS

TO THE

AGRICULTURAL SECTION

BY

R. H. REW, C.B.,

PRESIDENT OF THE SECTION.

Farming and Food Supplies in Time of War.

Agriculture is the antithesis of warfare; farming is pre-eminently a peaceful avocation, and farmers are essentially men of peace. The husbandman is not easily disturbed by war's alarms, and his intimate association with the placid and inevitable processes of Nature engenders a calmness of spirit which is unshaken by catastrophe. Many stories illustrative of this attitude of mind come to us from the battlefields. The complete detachment of the fighting men from the rest of the community which was usual up to quite recent times is impossible in these days when in almost every country the army is not a class but the nation. It is inconceivable now that a war could rage of which it could be said, as has been said of our Civil War: 'Excepting those who were directly engaged in the struggle, men seemed to follow their ordinary business and their accustomed pursuits. The story that a crowd of country gentlemen followed the hounds across Marston Moor, between the two armies drawn up in hostile array, may not be true; but it illustrates the temper of a large proportion of the inhabitants.'[1] But, while farmers and peasants within the range of the guns cannot now ignore the fighting, they have repeatedly demonstrated their invincible determination that the madness of mankind shall not interrupt the calm sanity of the ordered cultivation of the soil. Of a district in the Argonne, a correspondent, writing in April last, said: 'The spring seed has already been sown or is being sown, sometimes indifferently, under shell-fire, right up to the edge of the trenches.'[2] A story was told of a farmer in Flanders looking over the parapet of a trench and demanding of an indignant British officer whether any of his men had stolen his pig. On receiving a suitable reply, he observed that he had already asked the French, who also denied all knowledge of the missing animal, so that he supposed it must be those condemned Germans, whom he forthwith proceeded to interview. Such a sublime sense of values, such absorption in the things that matter, such contempt for the senseless proceedings of warfare, are only possible to the agriculturist. The quarrels of mankind are transient, the processes of Nature are eternal. One thinks of Matthew Arnold's lines:

The East bowed low before the blast
In patient deep disdain;
She let the legions thunder past,
And plunged in thought again.

But, while the farmer is by instinct a pacifist, he is also, in a cause which rouses him, a doughty fighter. In that same Civil War to which so many were indifferent, the farmers of East Anglia, under Cromwell, changed the course of English history; and the thoroughness with which they turned their plough-

[1] Prothero, *English Farming, Past and Present,* p. 104.
[2] *Westminster Gazette,* April 30, 1915.

shares into swords is demonstrated by the fact that when they took to soldiering they put the nation for the first and only time under what is now termed militarism; that is, government controlled by the Army. In the last battle fought on English soil the yeomen and peasants of the West Country proved, amid the butchery of Sedgemoor, that bucolic lethargy can be roused to desperate courage. Indeed, through all our island story, since the English yeomen first broke the power of mediæval chivalry and established the supremacy of infantry in modern warfare, it has been from the rural districts that the nation has drawn its military strength. Even in the present war, when the armies of the Empire have been drawn from all classes of the community, the old county regiments and the yeomanry squadrons with their roots in the countryside have proved once more that the peaceful rustic is as undismayed on the field of battle as on the fields of peace.

It is, however, in his pacific rather than in his belligerent aspect that the British farmer now claims our attention, and, before considering the position of farming in the present war, we may briefly glance at its position when a century ago the nation was similarly engaged in a vital struggle.

From February 1793 until 1815, with two brief intervals, we were at war, and the conflict embraced not only practically all Europe but America as well. The latter half of the eighteenth century had witnessed a revolution of British agriculture. The work of Jethro Tull, 'Turnip' Townshend, Robert Bakewell, and their disciples, had established the principles of modern farming. Coke of Holkham had begun his missionary work; Arthur Young was preaching the gospel of progress; and in 1803 Humphry Davy delivered his epoch-making lectures on agricultural chemistry. Common-field cultivation, with all its hindrances to progress, was rapidly being extinguished, accelerated by the General Inclosure Act of 1801. A general idea of the state of agriculture may be obtained from the estimates made by W. T. Comber of the area in England and Wales under different crops in 1808. There were then no official returns, which, indeed, were not started until 1866; but these estimates have been generally accepted as approximately accurate and are at any rate the nearest approach we have to definite information.

I give for comparison the figures from the agricultural returns of 1914, which approximately correspond to those of the earlier date:

—	1808	1914
	Acres	Acres
Wheat	3,160,000	1,807,498
Barley and rye	861,000	1,558,670
Oats and beans	2,872,000	2,223,642
Clover, rye-grass, &c.	1,149,000	2,558,735
Roots and cabbages cultivated by the plough	1,150,000	2,077,487
Fallow	2,297,000	340,737
Hop grounds	36,000	36,661
Land depastured by cattle	17,479,000	16,115,750

The returns in 1914 comprise a larger variety of crops than were cultivated in 1808. Potatoes, for instance, were then only just beginning to be grown as a field-crop, and I have included them, together with Kohl-rabi and rape, among 'roots and cabbages.'

The population of England and Wales in 1801 was 8,892,536, so that there were 35½ acres under wheat for every hundred inhabitants. In 1914 the population was 37,302,983, and for every hundred inhabitants there were 5 acres under wheat.

The yield of wheat during the twenty years ending 1795 was estimated at 3 qrs. per acre[3]; in 1914 it was 4 qrs. per acre. The quantity of home-grown wheat per head of population was therefore 8½ bushels in 1808, and 1½ bushels

[3] Report of Select Committee on the means of promoting the cultivation and improvement of the waste, uninclosed and unproductive lands of the Kingdom, 1795.

in 1914. Nevertheless, even at that time, the country was not self-supporting in breadstuffs. In 1810, 1,305,000 qrs. of wheat and 473,000 cwt. of flour were imported. The average annual imports of wheat from 1801 to 1810 were 601,000 qrs., and from 1811 to 1820 458,000 qrs. Up to the last decade of the eighteenth century England was an exporting rather than an importing country, and bounties on exports were offered when prices were low, from 1689 to 1814, though none were, in fact, paid after 1792.

During the war period we are considering, the annual average price of wheat ranged from 49*s.* 3*d.* per qr. in 1793 to 126*s.* 6*d.* per qr. in 1812; the real price in the latter year, owing to the depreciation of the currency, being not more than 100*s.* In 1814 the nominal price was 74*s.* 4*d.* and the real price not more than 54*s.* per qr.[4] The extent to which these high and widely varying prices were affected by the European war has been the subject of controversy. As we mainly depended on the Continent for any addition to our own resources, the diminished production during the earlier years in the Netherlands, Germany, and Italy, and in the later years of the war in Russia, Poland, Prussia, Saxony, and the Peninsula, reduced possible supplies. At the same time the rates of freight and insurance, especially in the later years of the war, increased very considerably. Tooke mentions a freight of 30*l.* per ton on hemp from St. Petersburg in 1809. On the other hand, a powerful impetus was given to home production, which was stimulated by Government action and private enterprise. Inclosure was encouraged by the General Inclosure Act of 1801, and 1,934 Inclosure Acts were passed from 1793 to 1815. The schemes for increasing and conserving food supplies were various. The Board of Agriculture, for example, offered prizes of 50, 30, and 20 guineas respectively to the persons who in the spring of 1805 cultivated the greatest number of acres—not less than 20—of spring wheat.[5] In 1795 a Select Committee recommended that bounties should be granted to encourage the cultivation of potatoes on 'lands at present lying waste, uncultivated, or unproductive,' and that means should at once be adopted to add at least 150,000 and perhaps 300,000 acres to the land under cultivation 'as the only effectual means of preventing that importation of corn, and disadvantages therefrom, by which this country has already so deeply suffered.' Another view of importation is presented by Tooke, who, in a discussion of the effect of the war, says: 'Although the war cannot have been said to have operated upon the supply of agricultural produce of our own growth and other native commodities, sufficiently to outweigh the circumstances favourable to reproduction, it operated most powerfully in increasing the cost of production and in obstructing the supply of such commodities as we stood in need of from abroad. It is therefore to war chiefly as affecting the cost of production and diminishing the supply, by obstructions to importation, at a time when by a succession of unfavourable seasons our own produce became inadequate to the average consumption, that any considerable proportion of the range of high prices is to be attributed.'[6]

The main cause of high prices and scarcity was the failure of the harvests. Mr. Prothero thus analyses the wheat harvests of the twenty-two years 1793-1814: 'Fourteen were deficient; in seven out of the fourteen the crops failed to a remarkable extent, namely in 1795, 1799, 1800, 1809, 1810, 1811, 1812. Six produced an average yield. Only two, 1796 and 1813, were abundant; but the latter was long regarded as the best within living memory.'[7]

It appears paradoxical, but in a sense it is true, to say that the scarcity of wheat in certain years arose from the fact that the country was too largely dependent on its own crop. The risk of a bad harvest in a climate such as that of the British Isles must always be serious, and by the fortune of war this risk between 1793 and 1814 turned out to be very high. When supplies are drawn from the four quarters of the globe, it is evident that the risk of a shortage in time of peace is greatly reduced. Whether in a great war it is preferable to be more dependent on the sea than on the season is debatable.

[4] Porter's *Progress of the Nation*, by F. W. Hirst, p. 183.
[5] *Annals of Agriculture*, 1805.
[6] *History of Prices*, ed. 1838, vol. i. p. 116.
[7] *English Farming, Past and Present*, p. 269.

In comparison with wars for national existence, such as that against Napoleon and in a still sterner sense that in which we are now engaged, other conflicts appear insignificant. The Crimean War, however, did affect our food supplies and had a reflex action on British agriculture. The cessation of imports from Russia caused a rise in the price of corn The average price of wheat rose to 72*s*. 5*d*. per qr. in 1854, 74*s*. 8*d*. in 1855, and 69*s*. 2*d*. in 1856. Only once before (in 1839) during the previous thirty-five years had it risen above 70*s*. There were then no agricultural returns, but the estimates of Lawes, which were generally accepted, put the area under wheat at a little more than 4,000,000 acres, a higher figure than has been suggested for any other period. It is, indeed, highly probable that the Crimean War marked the maximum of wheat cultivation in this country. It was a time of great agricultural activity and of rapid progress. To their astonishment, farmers had found, after an interval of panic, that the Repeal of the Corn Laws had not obliterated British agriculture and that even the price of wheat was not invariably lower than it had often been before 1846. Caird had preached 'High Farming' in 1848 and found many disciples, capital was poured into the land, and the high prices of the Crimean period stimulated enterprise and restored confidence in agriculture.

To generalise very roughly, it may be said that while the Napoleonic wars were followed by the deepest depression in agriculture, the Crimean War was followed by a heyday of agricultural prosperity which lasted for over twenty years. What the agricultural sequel to the present war may be, I leave to others to estimate, and I turn to consider briefly some of its effects on British farming up to the present time.

Harvest had just begun when war broke out on August 4: indeed, in the earlier districts a good deal of corn was already cut. The harvest of 1914 was, in fact, with the exception of that of 1911, the earliest of recent years, as it was also one of the most quickly gathered. The agricultural situation may perhaps be concisely shown by giving the returns of the crops then in hand, *i.e.*, in course of gathering or in the ground, with the numbers of live stock as returned on farms in the previous June The figures are for the United Kingdom, and I add the average for the preceding ten years for comparison :

		1904-13
	Qrs.	Qrs.
Wheat	7,804,000	7,094,000
Barley	8,066,000	7,965,000
Oats	20,664,000	21,564,000
Beans	1,120,000	1,059,000
Peas	374,000	525,000
	Tons	Tons
Potatoes	7,476,000	6,592,000
Turnips and swedes	24,196,000	26,901,000
Mangold	9,522,000	9,934,000
Hay	12,403,000	14,148,000
	Cwts.	Cwts
Hops	507,000	354,000
	No.	No.
Cattle	12,185,000	11,756,000
Sheep	27,964,000	29,882,000
Pigs	3,953,000	3,805,000
Horses	1,851,000	2,059,000

Farmers had thus rather more than their usual supplies of nearly every crop, the chief deficiencies being in peas, roots, and hay. The shortage of the hay-crop was, however, in some measure made up by the large stocks left from the unusually heavy crop of 1913. It was fortunate from the food-supply point

of view that two of the most plentiful crops were wheat and potatoes. The head of cattle was very satisfactory, being the largest on record, and pigs were well above average. Sheep, always apt to fluctuate in numbers, were much below average, the total being the smallest since 1882 with the exception of 1913.

On the whole, it was a good year agriculturally, and the supply of home-grown produce at the beginning of the war was bountiful. Nature at any rate had provided for us more generously than we had a right to expect.

At first it appeared as if farmers were likely to be sufferers rather than gainers by the war. Prices of feeding-stuffs, especially linseed and cotton-cakes, maize-meal, rice-meal, and barley-meal, rose at once, recruiting affected the labour supply, and difficulties arose in the distribution of produce by rail. With one or two exceptions, such as oats, the prices of farm produce showed but little rise for three or four months after the war began. Wheat rose about 10 per cent , barley remained about normal, cattle by November had not risen more than 3 per cent., sheep and veal-calves showed no rise until December, while poultry was actually cheaper than usual, though eggs rose considerably. Butter rose slightly, and cheese remained about normal. Up to nearly the end of the year, in fact, it may be said generally that British farm-produce made very little more money than usual.

Meanwhile the nation began to take a keen interest in the agricultural resources of the country, and farming became the object of general solicitude. We started with great energy to improvise, in truly British fashion, the means of facing the supreme crisis of our fate, but the elementary fact at once became obvious that it is impossible to improvise food. The main farm-crops take an unreasonably long time to grow, even if the land is prepared for them, and a sudden extension of the area under cultivation is not a simple proposition. It was freely pointed out—with undeniable truth—that our agricultural system had not been arranged to meet the conditions of a great European war, and many suggestions were made to meet the emergency. Some of these suggestions involved intervention by legislative or administrative action. It was decided that any attempt violently to divert the course of farming from its normal channels would probably not result in an increased total production from the land. The Agricultural Consultative Committee, appointed by the President of the Board of Agriculture on August 10, issued some excellent advice to farmers as to their general line of policy and the best means by which they could serve the nation, and this was supplemented by the Board and by the agricultural colleges and local organisations throughout the country. No less than thirty special leaflets were issued by the Board, but, while it may, I think, fairly be claimed that all the recommendations made officially were sound and reasonable, I should be the last to aver that farmers were universally guided by them. They do not accept official action effusively :

'Unkempt about those hedges blows
An English unofficial rose,'

and official plants do not flourish naturally in farm hedgerows. It was, however, fairly evident that patriotism would suggest an effort to obtain the maximum production from the land, and there were good reasons to think that self-interest would indicate the same course. It must be admitted, however, that during the autumn the lure of self-interest was not very apparent. Food-prices, however, at the end of the year began to rise rapidly. English wheat in December was 25 per cent. above the July level, in January 45 per cent., in February and March 60 per cent., and in May 80 per cent. Imported wheat generally rose to a still greater extent, prices in May standing for No. 2 North Manitoba 95 per cent., and No. 2 Hard Winter 90 per cent. above July level. The greater rise in imported wheat may be noted as vindicating farmers against the charge which was made against them of unreasonably withholding their wheat from the market. Cattle and sheep rose more slowly, but in March prices of both had risen by 20 per cent., and in May and June cattle had risen by about 40 per cent. Butter rose by about 20 per cent. and cheese by about 40 per cent. Milk rose little through the winter, but when summer contracts were made prices remained generally at the winter level.

British agriculture, like the British Isles, is a comparatively small affair

geographically. The 47 million acres which it occupies, compared with the 80 million acres of Germany or the 90 million acres of France, and still more with the 290 million acres of the United States, represent an area which may be termed manageable and about which one might expect to generalise without much difficulty. But, in fact, generalisation is impossible. Even on the 27 million acres of farm land in England and Wales there is probably more diversity to the square mile than in any country on earth. The variations in local conditions, class of farming, and status of occupier preclude the possibility of making any general statement without elaborate qualifications. Thus whatever one might say as to the effects of the war on agriculture would be certain to be inaccurate in some districts and as regards some farmers.

There are three main agricultural groups, corn-growing, grazing, and dairying. They overlap and intermingle indefinitely, and there are other important groups, such as fruit-growing, vegetable-growing, hop-growing, &c., which represent a very large share of the enterprise and capital engaged on the land. The receipts of the corn-growing farmer generally speaking were substantially increased. Probably about 50 per cent. of the wheat-crop had been sold before prices rose above 40*s.* per quarter, and there was very little left on the farms when they reached their maximum in May. Oats rose rather more quickly, but did not reach so high a level, relatively, as wheat. Barley—owing perhaps to enforced and voluntary temperance—never made exceptional prices, and in fact the best malting barleys were of rather less than average value. There is no doubt, however, that farmers who depended mainly on corn-growing found an exceptionally good market for their crops and made substantial profits. Farmers who depended mainly on stock were less generally fortunate, although stock were at a fairly high level of price when the war began. Sheep for some time showed no signs of getting dearer, but in the spring prices rose substantially, and a good demand for wool—which in one or two cases touched 2*s.* per lb.—made the flockmasters' returns on the whole very satisfactory. Cattle followed much the same course; stores were dear, but by the time fat stock came out of the yards or off the grass prices had risen to a very remunerative level. The large demands on imported supplies of meat for the British and French armies occasioned a distinct shortage for the civil population, but this was relieved by a reduced demand, so that the effect upon prices of native beef and mutton was not so great as might have been expected. The influence of a rise of price upon demand is more marked in the case of meat than in that of bread. While there has been a distinct reduction in the consumption of meat, there is no evidence of a reduced consumption of bread.

Dairy farmers generally found themselves in difficulties. Prices of butter and cheese increased but slightly, and milk remained for a considerable period almost unchanged. The rise in the prices of feeding-stuffs and the loss of milkers aggravated their troubles. An actual instance of the position in February as affecting a fairly typical two-hundred acre farm may be quoted. It had thirty milch cows producing about 16,500 gallons per annum. The cake bill showed an advance of fifty per cent., and wages had risen twelve per cent. It was calculated that the extra cost was 1·3*d.* per gallon of milk. Later the prices of milk, butter, and cheese rose, but on the whole it cannot be said that dairy farmers generally made exceptional profits.

While it is certain that the gross receipts by farmers were substantially increased, it is very difficult to estimate what the net pecuniary gain to agriculture has been. It can only be said generally that while some have made substantial profits, which were probably in very few cases excessive, many others have on balance (after allowing for extra cost) done no better financially, and some perhaps even worse, than in an average year of peace. With regard to one item of extra cost, that of labour, it is possible to make an approximate estimate. Agricultural labourers were among the first to respond to the call for the new armies, and, up to the end of January, fifteen per cent. had joined the forces of the Crown. This considerable depletion of labour was not acutely felt by farmers during the winter, but during the spring and summer serious difficulty was experienced and many devices were suggested—some of which were adopted—for meeting it. Naturally the wages of those agricultural labourers who were left rose, the rise varying in different districts but being

generally from 1*s*. 6*d*. to 3*s*. per week. Owing to the rise in the price of commodities, this increase of wages cannot be regarded as a profit to the labourers, but it is, of course, an outlay by farmers, which in England and Wales may be reckoned as amounting to an aggregate of about 2,000,000*l*.

This country has never suffered from a dearth of agricultural advisers, and in such a time as the present, when everyone is anxious to help the country, it is natural that they should be unusually plentiful. Advice was freely offered both to the Government how to deal with farmers and to farmers how to deal with the land. Whether in consequence of advice or in spite of it, it may fairly be said that farmers throughout the United Kingdom have done their duty. They have met their difficulties doggedly and have shown an appreciation of the situation which does credit to their intelligence. It was not easy last autumn when farmers had to lay their plans for the agricultural year to forecast the future. We were all optimists then, and many thought that the war might be over before the crops then being planted were reaped. It was clear, however, that the national interest lay in maintaining and, so far as possible, increasing the produce of the land. In the quiet, determined way which is characteristic of them, farmers devoted themselves to the task, and the returns recently issued give the measure of their achievement. They have added twenty-five per cent. to the acreage of wheat and seven per cent. to the acreage of oats, and they have kept the area of potatoes up to the high and sufficient level of the previous year. These are the three most important crops. They have also not only increased the stock of cattle, which was already the largest on record, but, in spite of unfavourable conditions and a bad lambing season, they have increased the stock of sheep. In view of these facts, I venture to say that British and Irish farmers have shown both patriotism and intelligence, and may fairly claim to have contributed their share to the national effort.

The share of British agriculture in the food supply of the nation is more considerable than is sometimes realised. When I last had the honour to address the British Association I ventured to emphasise this point, and I may be allowed to repeat, in a somewhat different form and for a later period, the figures then given. Taking those articles of food which are more or less produced at home, the respective proportions contributed by the United Kingdom, the rest of the Empire, and foreign countries were on the average of the five years 1910-14 as follows :

—	United Kingdom	British Empire Overseas	Foreign Countries
	Per Cent.	Per Ce	Per Cent.
Wheat	19·0	39·3	41·7
Meat	57·9	10·7	31·4
Poultry	82·7	0·2	17·1
Eggs	67·6	0·1	32·3
Butter (including margarine)	25·1	13·3	61·6
Cheese	19·5	65·4	15·1
Milk (including cream)	95·4	0·0	4·6
Fruit	36·3	8·	55·4
Vegetables	91·8	1·1	7·1

The war has directly affected some of our food supplies by interposing barriers against the exports of certain countries. Fortunately we were in no way dependent for any of these foods upon our enemies, though Germany was one of our main sources of supply for sugar. We received some small quantities of wheat or flour and of eggs from Germany, Hungary, and Turkey, some poultry from Austria-Hungary, and some fruit from Germany and Turkey, but the whole amount was insignificant. The practical cessation of supplies from Russia was the most serious loss, as we drew from thence on an average 9 per cent. of our wheat, 9 per cent. of our butter, and 16 per cent. of our eggs.

The rather humiliating panic which took possession during the first few days of the war of a certain section of the population, who rushed to accumulate stores of provisions, arose not only from selfishness but from insufficient appreciation of the main facts about food supplies. Our large imports of food are constantly dinned into the ears of the people, but the extent and possibilities of our native resources are practically unknown. It is very natural, therefore, that the man in the street should assume that even a temporary interruption of oversea supplies would bring us face to face with famine.

Within the first few days of the war, the Government, through the Board of Agriculture, obtained returns not only of the stocks of all kinds of food-stuffs in the country but also of the stocks of feeding-stuffs for animals and of fertilisers for the land. Powers were taken under the Articles of Commerce (Returns, &c.) Act to compel holders of stocks to make returns, but it is due to the trading community to say that in only two instances, so far as the Board of Agriculture was concerned, was it necessary to have recourse to compulsion. The returns of stocks of food-stuffs, feeding-stuffs, and fertilisers have been made regularly to the Board of Agriculture[8] every month since the outbreak of war, and the loyal co-operation of the traders concerned deserves cordial recognition by those whose official duty has been rendered comparatively easy by their assistance. I may be allowed to add that the readiness with which traders communicated information which was, of course, of a very confidential nature, displayed a confidence in Government Departments which they may regard with some satisfaction.

A very casual glance at the national dietary suffices to show that John Bull is an omnivorous feeder, and as the whole world has eagerly catered for his table his demands are exigent. But, for various reasons, our daily bread, reluctant though most of us would be to be restricted to it, is regarded as the measure and index of our food supplies. On the 4th of August the Board of Agriculture published an announcement that they estimated the wheat-crop then on the verge of harvest at 7,000,000 quarters, and that, including other stocks in hand, there was at that time sufficient wheat in the country to feed the whole population for four months; and a few days later, having then obtained further information from about 160 of the principal millers, they stated that the supplies in the country were sufficient for five months' consumption. The Board also announced, on August 5, that the potato crop would furnish a full supply for a whole year's consumption without the necessity for any addition from imports. When it was further announced that the Government had taken steps to ensure against a shortage of sugar it began to be generally realised that at any rate the country was not in imminent danger of starvation. Indeed, on a broad survey of the whole situation, it was apparent that our native resources, together with the accumulated stocks of various commodities held in granaries, warehouses, and cold stores, would enable the United Kingdom to face even the unimaginable contingency of a complete blockade of all its ports for a considerable period.

Nevertheless it was abundantly evident, not only to the man in the street, but even to those whose duty it was to consider such matters, that the maintenance of regular supplies was essential to avoid undue depletion of stocks. The risk that a certain number of vessels carrying food to this country might be sunk by the enemy was obvious, and it was at first very difficult to measure it. After a year of strenuous endeavour by the enemy it is satisfactory to record that, although a few cargoes of food-stuffs have been sunk, the effect on supplies has been practically negligible.

Under these circumstances it appeared that, provided adequate protection were given against unusual risks, commercial enterprise might in the main be relied upon to supply the demands of the people in the normal manner and in the usual course of business. It is a self-evident axiom that it is better not to interfere in business matters unless there is a paramount necessity for interference.

The machinery of modern business in a highly organised community is very

[8] Returns in Scotland and Ireland are made to the Agricultural Departments of those countries and the results transmitted to the Board of Agriculture and Fisheries.

complicated; the innumerable cog-wheels are hidden while the machine is running normally, but every single one of these becomes very obvious when you attempt to introduce a crowbar. With one or two exceptions the purveyors of food to the nation were left to conduct their business without official interference, though the Board of Trade took steps to ascertain what were the retail prices justified by the wholesale conditions and to disseminate the information for the protection of consumers against unreasonable charges.

One measure of a drastic and widespread nature was adopted. The exportation of a large number of commodities was prohibited. This was done for two reasons: (1) to conserve stocks in this country, and (2) to prevent goods from reaching the enemy. The latter object could be attained only very partially by this method so long as any sources of supply other than the ports of the United Kingdom were open to the enemy or to adjoining neutral countries. The former object—with which we are now only concerned—was on the whole achieved. The Board of Agriculture, concerned for the maintenance of our flocks and herds, at once secured a general prohibition of the exportation of all kinds of feeding-stuffs for animals. Many kinds of food-stuffs were at once included and later additions were made, so that for a long time past nearly all kinds of food have been included, though in some cases the prohibition does not apply to the British Empire or to our Allies. The exportation of fertilisers, agricultural seeds, binder twine, and certain other commodities more or less directly connected with the conservation of our food supplies, was also prohibited, so that generally it may be said that the outlet for any food in the country was under effective control. This is not the time or place to discuss the reasons why in some instances limited quantities of certain articles were allowed to escape under licence. It is only necessary to remark that in all such cases there were cogent reasons in the national interest for the action taken.

Direct Government intervention in regard to food supplies was limited to three commodities—sugar, meat, and wheat. In the case of sugar the whole business of supply was taken over by the Government—a huge undertaking but administratively a comparatively simple one, owing to the fact there are no home-grown supplies. Intervention in the meat trade was necessitated by the fact that the enormous demands of the Allied armies had to be met by drafts upon one particular kind of meat and mainly from one particular source. The Board of Trade co-operated with the War Office, and a scheme was evolved whereby a very large part of the output of meat from South America and Australia comes under Government control.

As regards wheat, the intervention of the Government took two forms. The scheme whereby the importation of wheat from India was undertaken by the British Government, in co-operation with the Indian Government, arose primarily from conditions in India rather than from conditions in the United Kingdom, although it is hoped and believed that the results will prove to be mutually advantageous. Other than this the intervention of the Government in regard to wheat was devised as an insurance against the risk of interruption of normal supplies, its main object being to prevent the stocks of wheat in the country from falling to a dangerous level at a time when the home crop would be practically exhausted. When the home crop is just harvested there are ample reserves in the country for some months, and, as the United States and Canada are at the same time selling freely, stocks held by the trade are usually high. While home-grown wheat remains on the farms it is practically an additional reserve supplementary to the commercial reserves. When it leaves the farmers' hands, even although it may not actually go into consumption, it becomes part of the commercial reserve. This reserve in the nature of business tends to be constant, but fluctuates within rather wide limits under the influence of market conditions. If the price of wheat rises substantially and the capital represented by a given quantity increases, there is a natural tendency to reduce stocks. If also there is any indication of a falling market ahead, whether from favourable crop prospects or the release of supplies now held off the market for any reason, a prudent trader reduces his stocks to the smallest quantity on which he can keep his business running. So long as shipments reach this country, as in normal times they do, with, as a member of the Baltic once expressed it to me, 'the regularity of buses running down Cheapside,' the

country may safely rely on receiving its daily bread automatically. But if any interruption occurred at a time when the trade, for the reasons just indicated, happened to be running on low stocks, the margin for contingencies might be insufficient. I am, of course, debarred from discussing the method adopted or the manner in which the scheme was carried out, but, as the cereal year for which it was devised is over, it is permissible to state that the object in view was successfully achieved.

Of the 47,000,000 people who form the population of the United Kingdom the large majority are absolutely dependent for their daily food on the organisation and regular distribution of supplies. The countryman, even if he possesses no more than a pig and a garden, might exist for a short time, but the town-dweller would speedily starve if the organisation of supplies broke down. He does not, perhaps, sufficiently realise the intricacy of the commercial arrangements which make up that organisation, or the obstacles which arise when the whole economic basis of the community is disturbed by a cataclysm such as that which came upon us thirteen months ago. The sorry catchword 'Business as usual' must have sounded very ironically in the ears of many business men confronted with unforeseen and unprecedented difficulties on every side. The indomitable spirit with which they were met, the energy and determination with which they were overcome, afford further evidence of that which has been so gloriously demonstrated on land and sea, that the traditional courage and grit of the British race have not been lost.

To the question how have our oversea food supplies been maintained during the first year of the war, the best answer can be given in figures.

Imports of the principal kinds of food during the first eleven months of the war were as under, the figures for the corresponding period of 1913-14 being shown for comparison:

—	1914-15	1913-14	Increase + or Decrease — per Cent.
	Thousands of Cwts.	Thousands of Cwts.	
Wheat (including flour)	113,797	113,398	— 1·39
Meat	15,868	18,026	—11·97
Bacon and hams	7,452	5,975	+24·72
Cheese	2,766	2,386	+15·93
Butter (including margarine)	5,376	5,748	— 6·47
Fruit	18,830	17,512	+ 7·53
Rice	9,573	4,840	+97·79
Sugar	35,029	38,356	— 8·67

In total weight of these food-stuffs, the quantity brought to our shores was rather larger in time of war than in time of peace. Yet one still occasionally meets a purblind pessimist who plaintively asks what the Navy is doing. This is a part of the answer. It is also a measure of the success of the much-advertised German 'blockade' for the starvation of England. So absolute a triumph of sea-power in the first year of war would have been treated as a wild dream by the most confirmed optimist two years ago. The debt which the nation owes to our sailor-men is already immeasurable. That before the enemy is crushed the debt will be increased we may be assured. The crisis of our fate has not yet passed, and we may be called upon to meet worse trials than have yet befallen us. But in the Navy is our sure and certain hope.

'That which they have done is but earnest of the things that they shall do.'

Under the protection of that silent shield the land may yield its increase untrodden by the invading foot, the trader may pursue his business undismayed by the threats of a thwarted foe, and the nation may rely that, while common prudence enjoins strict economy in husbanding our resources, sufficient supplies of food will be forthcoming for all the reasonable needs of the people.

Spottiswoode & Co. Ltd., Printers, London, Colchester and Eton.

British Association for the Advancement of Science.

SECTION B: MANCHESTER, 1915.

ADDRESS

TO THE

CHEMICAL SECTION

BY

PROFESSOR WILLIAM A. BONE, D.Sc., Ph.D., F.R.S.,

PRESIDENT OF THE SECTION.

Gaseous Combustion.

This year is, as many of you are doubtless aware, the centenary of Davy's invention of the Miner's Safety Lamp, which formed the starting-point of his brilliant researches upon Flame, in which he disclosed and brought within the range of experimental inquiry most of the intricate and baffling problems connected with the fascinating subject of gaseous combustion. Also, the ground on which we meet to-day is known to the whole scientific world as the place where, during more than a quarter of a century of continuous investigation, a succession of Manchester chemists, led and inspired by Professor H. B. Dixon, have devoted themselves to the elucidation of the many problems which Davy's work foreshadowed. Therefore, both in point of time and place, the occasion is singularly appropriate for a review of recent advances in this important field of scientific inquiry.

At the Sheffield Meeting of this Association in 1910 I had the honour of presenting to a joint conference of Section A and B (Physics and Chemistry) a Report summarising the then 'State of Science in Gaseous Combustion'[1] which gave rise to a keen and stimulating discussion, and was not only printed *in extenso* in the Annual Reports for that year, but was also widely circulated through the medium of the scientific and technical press. There is no need, therefore, for me to refer in any detail to the results of researches already dealt with in that Report. I can more usefully devote part of the time at my disposal to supplementing it with a review of more recent researches which have considerably extended our knowledge in many directions.

Ignition Phenomena.

The first Section of my 1910 Report was concerned with Ignition Temperatures and the Initial Phases of Gaseous Explosions, and it is in connection with ignition phenomena that subsequent progress has been most marked.

For the ignition of a given explosive mixture it is necessary that the temperature of its constituents should be raised, at least locally, to a degree at which a mass of gas self-heats itself by combination until it bursts into flame; or, in other words, to a degree at which the chemical action becomes autogenous or self-propelling, so that it quickly spreads throughout the whole mass. This particular degree, or in some cases range, of temperature is commonly spoken of as *the ignition-point* of the mixture, but in using the expression certain qualifications should be carefully borne in mind. In the first place, as H. B. Dixon and H. F. Coward had shown in 1909,[2] whereas when certain combustible gases (such, for example, as hydrogen and carbon monoxide, the mechanism of whose combustion is probably of a fairly simple character) and air or oxygen are *separately* heated in a suitable enclosure before being allowed to mix, the temperature at which ignition occurs lies within a very narrow

[1] *Brit. Assoc. Reports*, 1910 (Sheffield), pp. 469 to 505.
[2] *Trans. Chem. Soc.* 1909, vol. 95, pp. 514 to 543

range, which is, within the limits of experimental error, the same for both air and oxygen (*i.e.*, in the case of hydrogen it is 580° to 590°, and for carbon monoxide 640° to 658°); on the other hand, in cases where the mechanism of combustion is known to be very complex (*e.g.*, hydrocarbons), the ignition range is either fairly wide or is materially lower in oxygen than in air (or both), thus :

	In Air	In Oxygen
Methane	650°–750°	556°–700°

The explanation of such behaviour is probably to be sought in the known complexity of the combustion, and the marked tendency for appreciable and fairly rapid interaction between the inflammable gas and oxygen before the actual ignition-point is reached.

If, by any means, such preliminary interaction could be entirely suppressed, or if, on the other hand, it be very rapid in character, the observed 'ignition-range' would be narrowed, as is actually the case with ethylene (542° to 547° in air and 500° to 519° in oxygen).

There are two other means by which an explosive mixture may be ignited; one is by adiabatic compression and the other, and most commonly employed of all, is by the passage of an electric spark.

The adiabatic compression of an explosive mixture was originally suggested by Nernst as a means of determining its ignition-point, provided always (1) that ignition is not produced locally whilst the main of the gas is still far below the true ignition temperature, and (2) that the piston of the apparatus does not move appreciably after the gas has been raised to its ignition-point. At the time of my 1910 Report, Falk,[3] in America, had applied the method in the case of hydrogen and oxygen mixtures, with results which, in the light of more recent work, would appear to have been misleading or erroneously interpreted. Thus, for instance, he found that, of all the mixtures of hydrogen and oxygen, the equimolecular H_2+O_2 mixture has the lowest ignition-temperature (514°), from which he concluded that the gases react initially to produce hydrogen peroxide rather than steam. Such a conclusion, which I believe to be erroneous, naturally directed attention to the experimental method involved.

The subject was promptly taken up here in Manchester by H. B. Dixon and his co-workers,[4] with the result that much new light has been thrown on the phenomena accompanying ignition. The ratio of the ignition-temperature to the initial temperature of the mixture before compression, both expressed in degrees absolute $\frac{T_2}{T_1}$, may be calculated from the compression ratio, $\left(\frac{V_1}{V_2}\right)$, by means of the well-known formula for adiabatic compression :

$$\left(\frac{T_2}{T_1}\right) = \left(\frac{V_1}{V_2}\right)^{\gamma-1}$$

where $\gamma =$ the ratio of the specific heats at constant pressure and volume respectively of the mixture compressed, and which for a mixture of diatomic gases, such as hydrogen and oxygen, is usually taken as 1.40.

Dixon's recent photographic analysis of the appearance of flame when mixtures of carbon bisulphide and oxygen (CS_2+3O_2) are adiabatically compressed, have proved that the flame, starting from a point or layer, always takes an appreciable time to spread through the mixture, and that unless special precautions are taken to arrest the piston at the moment of attainment of the ignition condition, it may be driven in much further than the minimum distance for ignition. The real ignition-point, as above defined, is not necessarily synchronous with the actual appearance of flame; there may be, and usually is, an

[3] *Journ. Amer. Chem. Soc.* **28**, 1517; **29**, 1536.

[4] H. B. Dixon, L. Bradshaw, and C. Campbell, *Trans. Chem. Soc.* 1914, **105**, 2027; H. B. Dixon and J. M. Crofts, *ibid.* p. 2036.

appreciable '*pre-flame period*'; only in the fastest burning mixtures is this period negligible, and hence the necessity of stopping the movement of the piston artifically at the beginning of the period, a precaution which Falk seems to have neglected.

According to Dixon and Croft's recent determination by this method of the ignition-points of mixtures containing electrolytic gas, whereas successive additions of hydrogen or nitrogen progressively raise the ignition-temperature of the undiluted gas by regular increments, as would be supposed, successive additions of oxygen, on the other hand, lower it, as a glance at the following table will show:

The Ignition-points of Mixtures containing Electrolytic Gas by Adiabatic Compression.

By H. B. DIXON *and* J. M. CROFTS, 1914.

Electrolytic Gas, $2H_2 + O_2 = 526°$					
$+ xH_2$		$+ xN_2$		$+ xO_2$	
$x = 1$ . . .	544°	$x = 1$ . . .	537°	$x = 1$ . . .	511°
$x = 2$ . . .	561°	$x = 2$ . . .	549°	$x = 7$ . . .	478°
$x = 4$ . . .	602°	$x = 4$ . . .	571°	$x = 15$ . . .	472°
$x = 8$ . . .	676°	$x = 8$ . . .	615°		
$(526 + 18x)°$		$(526 + 11x)°$			

The observed raising effects of successive dilutions with hydrogen and nitrogen call for no comment, save that the relative greater effect of hydrogen, as compared with nitrogen, may be attributed to its greater thermal conductivity; but the lowering effect of oxygen is indeed puzzling, and its meaning can only be conjectured. Dixon and Crofts have suggested that it may be due either to the formation of some active polymeride of oxygen under the experimental conditions, which seem to me doubtful, or that the concentration of oxygen in some way or other brings about increased ionisation of the combustible gas. This at once raises the larger question of whether or not ignition is a purely thermal problem, as until recently has generally been supposed.

Professor W. M. Thornton, of Newcastle, has recently published some very suggestive work on the Electrical Ignition of Gaseous Mixtures,[5] which, quite apart from its theoretical interest, has an important bearing on the safety of coal-mines where electrical currents are used for signalling and other purposes.

The common belief that any visible spark will ignite a given explosive mixture of gas and air is, of course, quite erroneous, for, just as Coward and his co-workers have shown that for a given explosive mixture and sparking arrangement there is a certain limiting pressure of the gaseous mixture below which ignition will not take place, so from Thornton's work would it appear that a definite minimum of circuit energy is required before a given mixture at given pressure can be ignited by a spark. And, moreover, he has stated that the circuit energy required for the spark ignition of a given mixture say of methane and air is something like 56 times greater with alternating than with continuous current at the same voltage. From this he has argued that the igniting effect cannot be simply thermal, but must be in part at least ionic. This conclusion he has further supported with the statement that the igniting power of a unidirectional current is, in fact, proportional to the current in the case of many gaseous mixtures over an important part of their working range of inflammability.

While there is much that is suggestive in Thornton's work, there is also a good deal which seems very difficult to interpret from a chemical standpoint; I refer more particularly to his later supposition of 'stepped ignition,' which is based upon certain observed abrupt increases in the minimum igniting current required with condenser discharge sparks as the proportion of combustible gas

[5] *Proc. Roy. Soc.*, Sec. A, vol. 90 (1914), p. 272; *ibid.* vol. 91 (1914), p. 17.

in the air mixture examined progressively increases. In other words, it is claimed that continuous alteration of the proportions of gas and air in an explosive mixture is, or may be, accompanied by discontinuous alterations in the spark energy required for ignition. I must confess that, after careful examination of the published curves, I am quite at a loss to give them any chemical interpretation, and to being somewhat sceptical about this supposed 'stepped ignition.'

A repetition and extension of Professor Thornton's experiments would be most valuable as a means to a better understanding of the conditions of spark-ignition.

The Influence of Electrons upon Combustion.

During the discussion upon my 1910 report, Sir J. J. Thomson reminded chemists that combustion is concerned not only with atoms and molecules but also with electrons moving with very high velocities. They might be a factor of prime importance in such intensive forms of gaseous combustion as are realised in contact with hot or incandescent surfaces, as also in the explosion wave. It is known, of course, that incandescent surfaces emit enormous streams of electrons travelling with high velocities, and the actions of such surfaces may be due to the formation of layers of electrified gas in which chemical changes proceed with extraordinarily high velocities. Again, the rapidity of combustion in the explosion wave might (he thought) conceivably be due to the molecules in the act of combining sending out electrons with exceedingly high velocities, which precede the explosion-wave and prepare the way for it by ionising the gas.

With regard to this interpretation of the action of surfaces, Mr. Harold Hartley carried out a promising series of experiments in my laboratory at Leeds University upon the combination of hydrogen and oxygen in contact with a gold surface,[6] which lend some support to the idea, but they require further extension before it can be considered as finally proved. It is my intention in the near future to resume the systematic investigation of the matter as rapidly as circumstances permit; but the experimental difficulties are formidable, and the mere chemist working by himself may easily be misled. We badly need the active co-operation of physicists in elucidating the supposed *rôle* of electrons in combustion.

Professor H. B. Dixon and his pupils have, at Sir J. J. Thomson's suggestion, recently tested the idea as applied to the explosion-wave, with, however, negative results.[7] It is known, of course, that the motion of the ions can be stopped at once by means of a transverse magnetic field, in which they curl up and are caused to revolve in small circles, and the question which Professor Dixon decided to put to the test of experiment was whether the damping of the electronic velocities in a powerful magnetic field would have any appreciable effect upon either the initial phase of an explosion or upon the high velocity of detonation. But although he employed a very intense magnetic field, produced by powerful magnets specially constructed by Sir Ernest Rutherford for the deflection of electrons of high velocity, no appreciable effect was observed upon the character or velocity of the flame with any gas mixture at any stage of the explosion. And inasmuch as the high constant velocity of the explosion wave can be entirely accounted for on the theory of a compression-wave liberating the chemical energy as it passes through the gases, there seem to be as yet no experimental grounds for attributing it to the ionising action of electrons.

The Initial Period of 'Uniform Movement of Flame' through Inflammable Mixtures, and Limits of Inflammability.

Mallard and Le Chatelier, in their classical researches upon the combustion of explosive mixtures,[8] discovered that the propagation of flame when such a mixture is ignited in a horizontal tube differs according as whether the ignition

[6] *Proc. Roy. Soc.* 1914.
[7] *Proc. Roy. Soc.* 1914, Sec. A, vol. 90, p. 506.
[8] *Ann. des Mines*, 1883 (8), 4 274.

occurs near the open or closed end of the tube. In the first case, the flame proceeded for some distance down the tube at a practically uniform and fairly slow velocity, corresponding to the true rate of propagation 'by conduction'; this period of uniform movement is succeeded by an irregular oscillatory period, in which the flame swings backwards and forwards with increasing amplitudes, finally either dying out altogether or giving rise to detonation. With certain oxygen mixtures the initial period of uniform slow velocity was shorter and appeared to be abruptly succeeded by detonation, without the intervention of any oscillatory period. When, however, such mixtures were ignited near the closed end of a horizontal tube, the forward movement of the flame was continuously accelerated from the beginning, under the influence of reflected compression waves, until detonation was set up. Such, in general, was the sequence of the phenomena observed by these distinguished French investigators.

They proceeded to determine experimentally the velocities of the uniform slow movement of the flame in the case of a number of air and combustible gas-mixtures, and plotting their results (in cm. per sec.) as ordinates against percentages of inflammable gas as abscissæ, they obtained 'curves' which were in each case formed of two inclined straight lines converging upwards to a point which represented the composition and flame-velocity of the most explosive mixture. And they concluded that the points at which the downward production of the two lines met the zero velocity line would define the upper and lower limits of inflammability for the particular series of gas-air mixtures. Thus the curve they obtained for methane-air mixtures (fig. 1) showed a maximum velocity of 61 cm. per second for a mixture containing about 12·2 per cent. of methane, with lower and upper limits corresponding to 5·6 and 16·7 per cent. of methane, respectively.

An exact knowledge of the velocities of flame-propagation during this initial period of uniform slow movement, as well as of the limits of inflammability for mixtures of various combustible gases and air, is very important from a practical point of view. Makers of apparatus for burning explosive mixtures of gas and air want to know the speed of flame-propagation through such mixtures, not only at ordinary temperatures and pressures, but also when the mixtures are heated and used at higher pressures. Also it would be important to know whether or not in the case of a complex mixture of various combustible gases and air, where complete composition can be determined by analysis (as, for example, coal-gas and air), the velocity of flame-propagation can be calculated from the known velocities for its single components. Unfortunately, although more than thirty years have elapsed since Mallard and Le Chatelier's work was published, the necessary data are still wanting to answer such questions, and anyone who will systematically tackle the problem and carefully work it out in detail will be doing a real service to the gas-using industries. I am hoping shortly to make a beginning with such an investigation in my new Department at the Imperial College, London, but the successful and rapid progress of such work will involve considerable financial outlay as well as organisation and expert direction. Who will help us with the necessary funds?

An accurate knowledge of the behaviour of methane-air mixtures under known variations of conditions is of prime importance from the point of view of the safety of coal-mines, and it is rightly occupying the attention of my friend and former collaborator, Dr. R. V. Wheeler, at the Home Office Experimental Station at Eskmeals And from papers which he has already published, as well as from some unpublished results which he has very kindly permitted me to refer to in this Address, it is now possible to correct certain errors in Mallard and Le Chatelier's results, and to arrive at a clearer view of the phenomena as a whole.

In the first place, it would appear that the initial 'uniform movement' of flame in a gaseous explosion, or, in other words, propagation of the flame from layer to layer by conduction only (as defined by Le Chatelier), is a limited phenomenon, and is only obtained in tubes of somewhat small diameter, wide enough, however, to prevent appreciable cooling of the flame, but narrow enough to suppress the influence of convection currents Moreover, ignition must be either at, or within one or two centimetres of, the end of the tube or

otherwise—particularly with the more rapidly moving flames—vibrations may be set up from the beginning.

Whilst all methane-air mixtures develop an initial uniform slow flame-movement period when ignited at or near the open end of a horizontal tube, both its linear duration as well as the flame velocity are not, according to private information which Dr. Wheeler has sent me, independent of the dimensions of the tube (*vide* fig. 2). The speed of flame increases with the diameter of the tube, and the linear duration of the uniform period increases with both the diameter and length of the tube up to a certain maximum, after which increase in length probably makes no appreciable difference; also, for the same tube, it varies with the proportion of methane in the explosive mixture, being greater as the speed of the flame diminishes, until with the two 'limiting' explosive mixtures it appears to last almost indefinitely.

Dr. Wheeler's recent redetermination of the velocities of the flame move-

RATE OF INFLAMMATION.

(*Le Chatelier.*)

METHANE AND AIR.

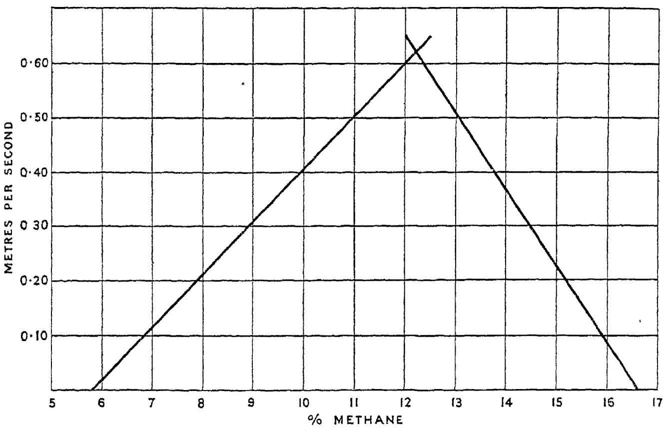


FIG. 1.

ment during this initial uniform period for mixtures of methane and air in varying proportions within the limits of inflammability has revealed serious errors in Mallard and Le Chatelier's original results for horizontal tubes of the same diameter as those which Dr. Wheeler has employed. Moreover, Mallard and Le Chatelier's method of determining the composition of the upper and lower limits of inflammability by extrapolation from their curves has been proved to be unwarranted. Dr. Wheeler considers that the length of the tubes used by Mallard and Le Chatelier (1 mètre only) was insufficient to ensure that the speed measurements of its initial uniform flame-movement period were uneffected by the subsequent 'vibratory period.' Also, the methane used by them, prepared as it was from sodium acetate, would obviously be impure.

The most important differences between the latest results published by Dr. Wheeler and those originally determined by Mallard and Le Chatelier, as shown on the accompanying diagram (fig. 3) are as follows:

(1) According to Wheeler, the limits of inflammability for horizontal propagation of flame in methane-air mixtures, at atmospheric temperature and pressure, correspond to 5·4 and 14·3 per cent. methane contents, respectively, whereas Mallard and Le Chatelier gave 5·6 and 16·7 per cent.

(2) Whereas, according to Mallard and Le Chatelier, the flame velocities for mixtures near the upper and lower limits would gradually approximate to the zero velocity ordinate, as the limiting composition was approached, according to Wheeler the velocities for both the upper and lower limiting mixtures are considerable (in each case about 36 cm. per sec.), and there is an abrupt change from these velocities to zero velocity as the particular limiting composition is passed.

Dr. R. V. Wheeler on Speeds of Uniform Movement of Flame through Methane-Air Mixtures in Tubes of Different Diameters.

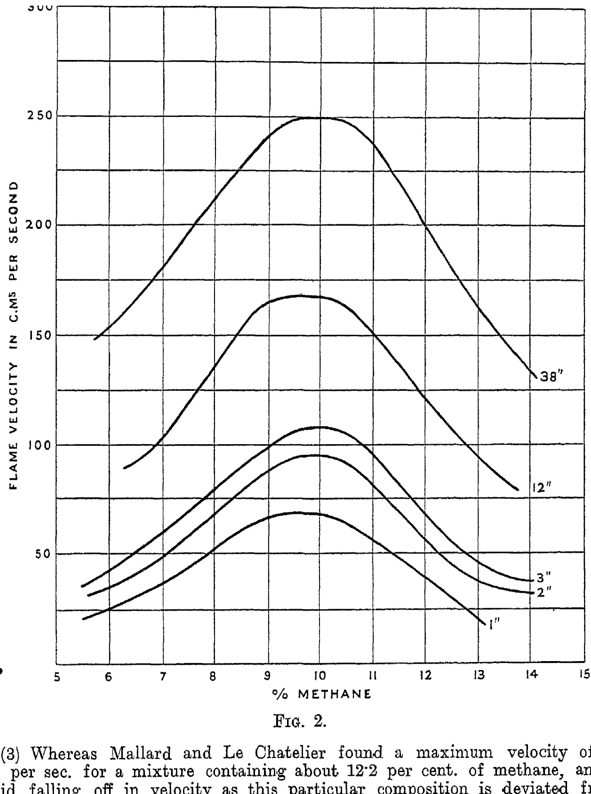


FIG. 2.

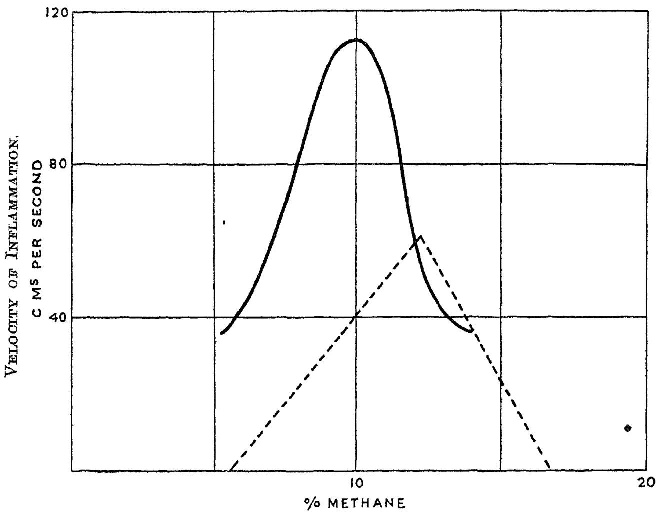
(3) Whereas Mallard and Le Chatelier found a maximum velocity of 63 cm. per sec. for a mixture containing about 12·2 per cent. of methane, and a rapid falling off in velocity as this particular composition is deviated from, Wheeler finds a maximum velocity of 110 to 112 cm. per second for a series of mixtures containing from 9·45 to 10·55 per cent. of methane. Such differences as are thus disclosed only emphasise the need of a complete experimental revision of the subject.

Messrs. Burgess and Wheeler have recently determined the limits of inflammability of methane when mixed, at atmospheric temperature and pressure, with 'atmospheres' of oxygen and nitrogen containing less oxygen than ordinary air. From their results (see table below) it would appear that as the oxygen content

of the atmosphere is reduced, the limits of inflammability are narrowed until they coincide when the oxygen content falls below 13·3 per cent., which means that an atmosphere containing 13·3 or less per cent. of oxygen is truly extinctive for a methane flame at ordinary pressures.

Atmosphere		Methane per cent.	
Oxygen	Nitrogen	Lower Limit	Higher Limit
20·90	79·10	5·60	14·82
17·00	83·00	5·80	10·55
15·82	84·18	5·83	8·96
14·86	85·14	6·15	8·36
13·90	86·10	6·35	7·26
13·45	86·55	6·50	6·70

My review of this part of the subject would be incomplete without a reference to some interesting observations which have been made by Dr. H. F. Coward and co-workers at the Manchester School of Technology upon the behaviour of weak mixtures of various inflammable gases and air at or just below the lower limit of inflammability in each case.[9] Their principal experiments were carried out in a rectangular box of 30 cm. square section and 1·8 metres length, with two opposite sides of wood, and the other two of plate glass. The box was placed in an upright position, the bottom being water-sealed and the



Burgess and Wheeler ——
Mallard and Le Chatelier

FIG. 3

top closed, with a suitable igniting device near the bottom. They have shown that caps or vortex rings of flame may be projected for some distance upwards from the source of ignition, sometimes apparently for an indefinite distance, without igniting the whole of the combustible mixture. In such mixtures

[9] *Trans. Chem. Soc.* 1914, **105**, p. 1859.

there may be an indefinite upward slow propagation of flame together with incompleteness of combustion (much of the combustible mixture remaining unburnt), and the question naturally arises as to how the term 'inflammability' should be scientifically defined. Dr. Coward has argued with some force that a gaseous mixture should not be termed 'inflammable' at a given temperature and pressure unless it will propagate flame indefinitely, the unburnt portion being maintained at that temperature and pressure. Inflammability, thus defined, would be a function of the temperature, pressure, and composition of a particular mixture only and would be independent of the shape and size of the containing vessel; and, provided that it is kept in mind that for each particular mixture at a given temperature and pressure a certain minimum igniting energy and intensity is requisite, I am inclined to agree with the definition. Also, there is the possibility that in a mixture just at or very near one or other of the limits of inflammability flame may be propagated *upwards* but not *downwards*.

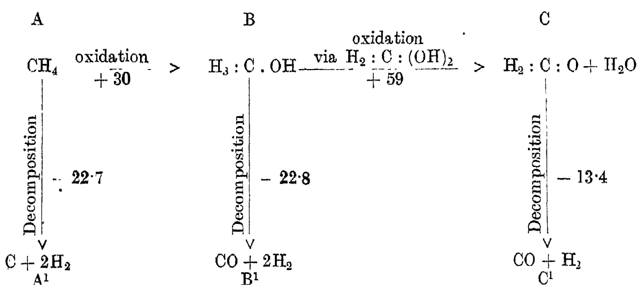
From his experiments Dr. Coward has assigned the following as the lower limits of inflammability of hydrogen, methane and carbon monoxide, respectively, in air at atmospheric temperature and pressure :

	Per cent.
Hydrogen	4·1
Methane	5·3[10]
Carbon monoxide . . .	12·6

The Combustion of Hydrocarbons and the Relative Affinities of Methane, Hydrogen, and Carbon Monoxide, respectively, for Oxygen in Flames.

Under the title of 'Gaseous Combustion at High Pressure' I have recently published, in conjunction with various collaborators,[11] a further instalment of my researches upon the mechanism of hydrocarbon combustion,[12] and I may perhaps be allowed to draw your attention to certain new points which have arisen in connection therewith.

A detailed study of the behaviour of mixtures of methane and oxygen of composition ranging between $2CH_4+O_2$ and CH_4+O_2, when exploded in steel bombs at initial pressures of 12·7 atmospheres (see Table, Fig. 4), has shown it to be consistent with the 'hydroxylation' theory of hydrocarbon combustion which I put forward some years ago as the result of my previous work. The following scheme seems to interpret correctly the chemical and thermal changes involved in the initial stages of the explosive combustion of methane :



[10] Too much stress need not be laid upon the difference between this number and the 5·6 per cent. given by Dr. Wheeler (*loc. cit.*), because Dr. Coward himself admits that the flames of mixtures containing from 5·3 to 5·6 per cent. of methane are very sensitive to shock, whilst a 5·6 per cent. mixture will always propagate flame indefinitely even when there is a moderate disturbance; the conditions must be exceedingly tranquil to prevent extinction in the other cases.

[11] Messrs. Hamilton Davies, H. H. Gray, H. H. Henstock, and J. B. Dawson.

[12] *Phil. Trans. Roy. Soc.*, A., vol. 215 (1915), pp. 275 to 318.

Explosion of Mixtures of varying Composition between $2CH_4 + O_2$ and $CH_4 + O_2$ in Bomb at Initial Pressure 12·7 atmospheres.

Experiment No.	10			11			12			13		
Pressures in atmospheres p_1	12·74			12·72			12·68			12·61		
Pressures in atmospheres p_2	15·05			18·10			17·30			14·10		
p_2/p_1	1·18			1·42			1·36			1·12		
	Per cent.	Partial Pressures, Atmospheres		Per cent.	Partial Pressures, Atmospheres		Per cent.	Partial Pressures, Atmospheres		Per cent.	Partial Pressures, Atmospheres	
Original mixture CH_4	67·15	8·55		59·3	7·54		55·75	7·09		50·8	6·40	
Original mixture O_2	32·85	4·19		40·7	5·18		44·25	5·59		49·2	6·21	
	Per cent.	Partial Pressures, Atmospheres		Per cent.	Partial Pressures, Atmospheres		Per cent.	Partial Pressures, Atmospheres		Per cent.	Partial Pressures, Atmospheres	
Gaseous products CO_2	3·10	0·47		2·70	0·49		3·85	0·67		7·45	1·05	
Gaseous products CO	25·40	3·83		35·60	6·44		37·25	6·44		38·50	5·44	
Gaseous products CH_4	16·60	2·50		4·65	0·84		2·25	0·39		nil	nil	
Gaseous products H_2	54·90	8·26		57·05	10·32		56·65	9·80		54·05	7·60	
	C.	H.	O.	C.	H.	O.	C.	H.	O.	C.	H.	O.
Units in original mixture	8·55	17·10	4·19	7·54	15·08	5·18	7·09	14·18	5·59	6·40	12·80	6·21
Units in gaseous products	6·80	13·26	2·38	7·77	12·00	3·71	7·50	10·58	3·89	6·49	7·60	3·77
Difference atmospheres	1·70	3·84	1·81	—	3·08	1·47	—	3·60	1·70	—	5·20	2·44
Difference per cent.	20		43·2			28·4			30·4			38·2
Ratio $\frac{CO \times OH_2}{CO_2 \times H_2}$ in products	3·65			3·75			3·43			3·4		
Remarks	Explosion almost inaudible; carbon deposited.			Explosion distinctly audible.			No carbon deposited.			Very violent explosion with sharp metallic click. No carbon deposited		

The principal experimental facts which this, or indeed any alternative, scheme must explain are as follows:

(1) That whenever mixtures of composition between $2CH_4+O_2$ and CH_4+O_2 are exploded under pressure a considerable proportion of the original oxygen appears as steam in the products;

(2) That there is a marked *minimum* in the proportion of such oxygen as the composition of the original mixture approximates to $3CH_4+2O_2$; and

(3) That there is a total cessation of any separation of carbon (which is very marked with mixtures $2CH_4+O_2$) after the proportion of oxygen in the original mixture attains or exceeds the limit $3CH_4+2O_2$.

Now, if, as I believe, the initial interaction of methane and oxygen is at all temperatures essentially a 'hydroxylation' process, accompanied by the decomposition (more or less rapid according to the temperature) of the primarily formed hydroxylated molecules, a consideration of the chemical and thermal aspects of the process will point to certain probabilities which are indeed actually realised in fact.

In the first place, monohydroxymethane (methyl alcohol) $CH_3.OH$ is known to decompose at high temperatures, yielding carbon monoxide and oxygen, without any separation of carbon or formation of steam, $CH_3.OH=CO+2H_2$. Also, the very unstable dihydroxy methane, $H_2:C:(OH)_2$ would yield formaldehyde and steam, $H_2:C:(OH)_2=H_2:C:O+H_2O$, and the formaldehyde would in turn decompose into carbon monoxide and hydrogen, $H_2:C:O:=CO+H_2$, without any deposition of carbon whatever.

If now the thermal consequences of such facts be considered, it would appear (1) that if the oxidation of methane to $H_3:C.OH$ (A to B in the scheme) were accompanied by thermal decomposition at this stage (B to B[1]), the *net* heat evolution would be $(30-22{\cdot}8)=7{\cdot}2$ kilogram Centigrade units; whereas (2) if the same amount of oxygen reacted in such a way that there was a 'non-stop' run through the *monohydroxy-* to the *dihydroxy-* stage, with decomposition at the point (A to C and C to C[1]), the corresponding net heat evolution would be $\frac{1}{2}(30+59-13{\cdot}4)=37{\cdot}8$ units, or about five times as much as in (1). Hence there would always be a strong tendency for such a non-stop run from A to C through B, without any decomposition occurring at B, and such would always occur whenever the oxygen present in the original mixture attained the equimolecular proportion CH_4+O_2.

Again, if the original mixture contained only half such proportion of oxygen $(2CH_4+O_2)$, there would still be a decided preference for an oxidation of one-half of the methane by a non-stop run A to C through B, rather than an oxidation of the whole of the methane to B only, the other half of the methane remaining unchanged, or undergoing thermal decomposition into its elements, $CH_4=C+2H_2$ (A to A[1]). Also, the latter process would use up no more of the energy developed from the oxidation than would be required for the decomposition of a corresponding quantity of $H_3:C.OH$ at stage B (B to B[1]). Hence when such a mixture, $2CH_4+O_2$, is exploded under pressure, the formation of carbon and its oxides, hydrogen, and considerable quantities of steam may be expected, which is what actually occurs.

When, however, the proportion of oxygen in the original mixture reaches the limit $3CH_4+2O_2$, whilst it is still insufficient to oxidise the whole of the hydrocarbon to the *dihydroxy-* stage, there is enough of it to prevent any methane remaining unoxidised to (at least) the *monohydroxy-* stage, and, therefore, seeing that the affinity of methane for oxygen far exceeds those of either hydrogen or carbon monoxide, it is to be expected that no substantial proportion of the original methane would escape oxidation to either the *mono-* or the *dihydroxy-* stage. But inasmuch as not more than about one-third of the original methane could, in the circumstances, be transformed into the di-hydroxy- stage, it follows that a considerable amount of thermal decomposition at the mono-hydroxy- stage would occur.

If this view is correct, it follows that there should be an entire suppression of carbon deposition at or about the $3CH_4+2O_2$ ratio, and, also, that with this

particular mixture a smaller proportion of the original oxygen should appear as steam in the products than would be the case with either the $2CH_4+O_2$ or the CH_4+O_2 mixture, which again is precisely what we find.

In considering the question of the explosive combustion of hydrocarbons it is important to distinguish between (1) the primary oxidation of the hydrocarbon, which is an exceedingly rapid process and is probably completed during the short interval between ignition and the attainment of maximum pressure, and (2) certain probable secondary interactions where influence may extend far into the subsequent cooling period, for it is only this latter which would be affected by variations in the rate of cooling down from the maximum temperature. Such secondary interaction may include (*a*) the reversible change $CO+OH_2 \rightleftharpoons CO_2+H_2$ and, in cases where carbon is deposited as the result of the decomposition of primary oxidation products, the interaction of steam and carbon $C+OH_2=CO+H_2$. In this connection, I may draw attention to the recently published work of G. W. Andrew,[13] one of my former pupils and collaborators, on the 'Water Gas Equilibrium in Hydrocarbon Flames' which proves that in a system containing only CO_2, CO, H_2, H_2O, rapidly cooling down from the high temperatures prevailing in hydrocarbon flames, the equilibrium ratio $\frac{CO \times OH_2}{CO_2 \times H_2}$ adjusts itself automatically with the temperature until a point between 1500° and 1600° C. on the cooling curve is reached (corresponding to a value $K=4{\cdot}0$ approximately), after which no further readjustment occurs. He also found that this adjustment of equilibrium is not greatly influenced even when relatively large quantities of methane and carbon are found in the final products.

I am able, from my own experiments, to confirm Andrew's conclusions in all cases where the initial firing pressure is insufficient to set up detonation; but, in cases where both detonation and separation of carbon occur, my results undoubtedly indicate an appreciable intervention of the separated carbon during the cooling period. There is nothing in my results, however, suggestive of an appreciable intervention of methane.

The fact that the primary oxidation of methane usually involves a direct transition from CH_4+O_2 to $CH_2(OH)_2$, which latter breaks up into, ultimately, $CO+H_2+H_2O$, without any deposition of carbon, opened up the possibility of instituting a direct experimental comparison between the relative affinities of methane and hydrogen for oxygen in explosions by exploding a series of mixtures $CH_4+O_2+xH_2$ in which the hydrocarbon and oxygen were initially present in as nearly as possible equimolecular proportions, but in which x (the volume ratio of H_2 to CH_4) was varied between 2 and 8, and determining (1) the oxygen distribution on explosion when $x=2$, and (2) the influence upon such distribution of successive equal increments of x up to 8.

Ever since Davy's experiments on Flame, the combustibility of hydrogen has been erroneously considered to be superior to that of methane; but, on the other hand, my previous researches upon hydrocarbon combustion have shown (1) that in slow combustion in borosilicate glass bulbs at temperatures between 300° and 400° C. methane, ethane, and acetylene are all oxidised at a much faster rate than is either hydrogen or carbon monoxide; and also (2) that in exploding such mixtures as $C_2H_4+H_2+O_2$ or $C_2H_2+2H_2+O_2$ the hydrocarbon is burnt in preference to hydrogen, facts which are at variance with any notion of the superior affinity of hydrogen for oxygen in flames.

My further experiments upon the relative affinities of methane and hydrogen for oxygen in explosives were carried out at high initial pressures in two special steel bombs, namely, A, with a cylindrical explosion chamber 8 inches long and 1 inch in diameter (capacity=*circa* 103 c.c.), and, B, with a spherical cavity 3 inches in diameter (capacity=*circa* 275 c.c.). The ratio of wall surface of the explosion chamber in the case of Bomb A would thus be about 2.75 times that of Bomb B. The mean results obtained for the distribution of oxygen

[13] *Trans. Chem. Soc.* 1914, **105**, pp. 444 to 456.

between methane and hydrogen in the two sets of experiments were as follows :

Distribution of Oxygen when Mixtures $CH_4+O_2+xH_2$ *are exploded.*

x	2	4	6	8
	%	%	%	%
Bomb A { O_2 to CH_4 . . .	95·34	81·0	54·9	31·4
Bomb A { O_2 to H_2 . . .	4·66	19·0	45·1	68·6
Bomb B { O_2 to CH_4 . . .	97·1	91·0	72·6	—
Bomb B { O_2 to H_2 . . .	2·9	9·0	27·4	—

It is at once evident from the results with the mixture $CH_4+O_2+2H_2$ that the affinity of methane is at least twenty to thirty times greater than that of hydrogen for oxygen in explosion flames. The actual distribution of oxygen when a particular mixture is exploded is undoubtedly influenced to some extent by the walls of the containing vessel, but not, so far as I have been able to ascertain, by the absolute initial pressure. Moreover, it is evident that the influences of successive increases in x, the volume ratio of H_2 to CH_4 in the mixture exploded, upon the actual oxygen distribution for a given explosion is proportionate to x^2, which can hardly mean other than that in explosion flames hydrogen is *directly* burnt to steam, and not, as many have supposed, *via* the intermediate formation of hydrogen peroxide.

A similar series of experiments with mixtures CH_4+O_2+xCO show that whilst it is impossible, owing to circumstances which I need not here detail, to deduce, even approximately, any numerical relation between the affinities of methane and carbon monoxide for oxygen in flames, yet we can confidently say that the affinity of the hydrocarbon is again vastly the superior, although carbon monoxide is apparently more effective than hydrogen in pulling away oxygen from the predominating attraction of methane.

Having thus satisfactorily disposed of the question of relative affinities of the three gases for oxygen in explosion flames, it remained to prove whether or not such chemical factors determine the ratio of attainment of maximum pressure in explosions, by exploding mixtures of each of the three combustible gases with air, in such proportions as correspond with the primary oxidation, namely : (1) $CH_4+O_2+4N_2$, (2) $2H_2+O_2+4N_2$, and (3) $2CO+O_2+4N_2$, at initial pressure of from 45 to 50 atmospheres in the spherical Bomb B to which was attached a Petaval recording manometer with its optical accessories.

The pressure records which are reproduced in the following diagrams (figs. 5, 6, and 7), in which pressures in atmospheres are plotted against time in seconds, prove conclusively the absence of any direct relation between the

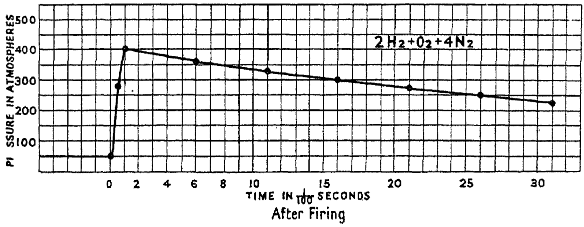


FIG. 5.

actual rate at which the potential energy of an explosive mixture is transferred on explosion as sensible heat to its products and the magnitude of the chemical affinity between its combining constituents. This is hardly to be wondered at,

seeing the extreme rapidity of chemical interaction at high temperature as compared with the rate at which the liberated heat can be communicated to and uniformly distributed amongst the products by ordinary physical processes. Attention may be drawn to the extreme slowness of the cooling in each case after the attainment of maximum pressure; this has also been observed by

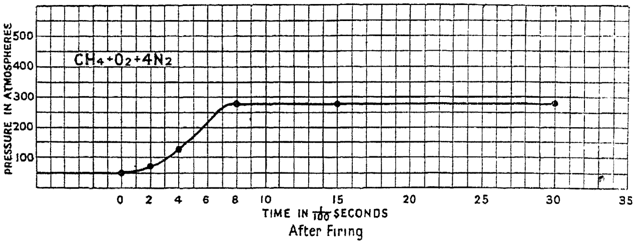


FIG. 6.

previous workers in other similar cases of gaseous explosions. This was particularly marked in the case of the methane-air mixture, in which there was hardly any appreciable cooling during an interval of 0·22 seconds after the attainment of maximum pressure (in 0·08 sec.), a circumstance which may be due in part to the combustion taking place in well-defined chemical stages, and

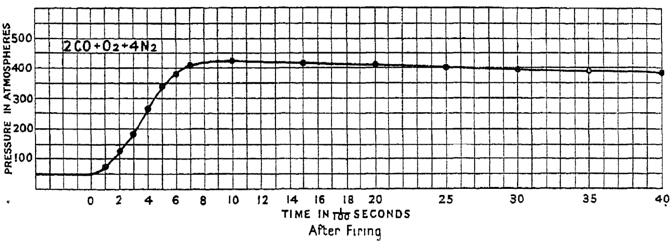


FIG. 7.

in part also to the operation of the exothermic secondary interaction between carbon monoxide and steam during the cooling period. On the other hand, the curve for the hydrogen-air mixture, where the combustion to steam is a direct and comparatively simple transaction, suggests that the attainment of maximum pressure is succeeded by a period of gradual cooling uninfluenced by chemical combination.

Fuel Economy and the Proper Utilisation of Coal.

Leaving now the scientific aspects of flame and combustion I wish to say a few words, as a technologist, upon the great national importance of a more adequate scientific control of fuel consumption and the utilisation of coal generally, with special reference to the situation created by this terrible and ruinous European conflict. And my remarks will be addressed in part to my chemical friends and colleagues, who are primarily interested in scientific research and its industrial applications, and in part also to the commercial and manufacturing community, which is chiefly interested in the financial results of such scientific activity.

Notwithstanding the fact that we are raising annually in the United Kingdom (according to the official estimate for 1913) 287 million tons of coal,

of which 189 million tons (or, say, 4 tons per head of population) were consumed at home more or less wastefully, it is indeed surprising how little has been done, or is being done, by the scientific community to impress upon the Government and the public generally the importance of establishing some systematic control or investigation of fuel consumptions in all large industrial areas. Deputations have waited upon the Government about the question of reviving our languishing coal-tar colour industry, so that in future we may be independent of Germany for the supply of the two million pounds' worth of dye-stuffs required by our textile industries, and already a State-aided organisation, with an advisory scientific committee, has sprung into existence to achieve that desirable result. But no organised body of scientific men, so far as I know, has ever thought it important, or worth while, to take an active interest in the vastly greater subject of fuel economy and the proper utilisation of coal, upon which the dyeing industry depends for its raw materials.

It is unnecessary for me to remind you that the contending armies in this Armageddon of the nations depend upon certain distillation products of coal for their supplies of high explosives, and there is little doubt in my mind but that Germany's violation of the neutrality of Belgium, and her subsequent seizure of that country and of a large tract of Northern France, had more than a purely political or strategic significance. She doubtless wanted also to seize for herself, and at the same time to deprive her enemies of, coal-fields lying just beyond her own borders, which are capable of furnishing abundant supplies of coal admirably adapted for yielding the raw materials for the manufacture of high explosives. A country in which all metallurgical coke has for years past been manufactured under chemical supervision in by-product coking ovens with recovery of ammonia, tar, and benzol, and in which the wasteful beehive coking ovens have long ago ceased to exist, was hardly likely to overlook the military importance of the Belgian coal-field with its many by-product coking plants. And, moreover, but for German commercial acumen and enterprise, during many years past, our own by-product coking industry would not have attained even to its present respectable dimensions. Certainly, it owes very little to the interest or attentions of British chemists, most of whom are, unfortunately, but little aware of its circumstances and conditions, and seem to care even less for its particular problems. And yet in proportion to the capital outlay upon it, it is one of the most profitable of all our chemical industries, coal-tar colour-making not excepted.

Fuel economy, and the proper utilisation of coal, whether in connection with manufacturing operations or with domestic heating, will become one of the most important national questions during the trying years that will follow hard upon this war, because, of all directions in which national economy can be most healthfully and advantageously exercised, this is perhaps the most obvious and prolific. For it is tolerably certain that with an efficient and systematic public supervision of fuel consumptions we ought to be able, even with existing appliances, to save many millions of pounds of our annual coal-bill, and with improved appliances still more millions, a saving which would in the long run redeem a considerable amount of the War-loan, which has been much more easily raised than it will be repaid.

Now, I fear that not only are chemists for the most part lamentably ignorant of the nature of coal, and of modern fuel technology, but they have been for many years past so indifferent about such questions as to leave them almost entirely to engineers, who, as a body, are notoriously deficient in chemical sense and experience. The engineer has indeed not usurped the place of the chemist, but has had to do his best to fill the position long since abdicated by the chemist.

This, indeed, seems strange when we remember that the foundations of modern chemistry were deeply laid by investigators who were, above all things, 'fire-worshippers.' But, judging from most chemical text-books, nearly all that the modern student of chemistry is taught in our academies about combustion was known to Lavoisier, and I question whether in the majority of our university laboratories any investigation upon coal or combustion is ever undertaken. And yet the subject is full of the most fascinating and fundamental theoretical problems, for the most part unsolved, and the nation consumes every week as much coal as could be exchanged for the whole quantity of aniline dyes used by its textile industries in a year.

Moreover, such advances as have been made during recent years, and they are by no means inconsiderable, have nearly all been in the direction of the wider applications of gaseous fuels, yet in how many of our University laboratories is even gas analysis taught, or how many of our Schools of Chemistry provide systematic courses in the chemistry and manipulation of gases, without which no professional training of industrial chemists, however much 'research work' it may include, ought to be considered satisfactory? It is my opinion that this important branch of our chemical craft and science has not, for many years past, been accorded its proper place and share of attention in the ordinary curriculum of the majority of our academic institutions.

Of the 189 millions of coal consumed in the United Kingdom in the year 1913, about 40 million tons, or say approximately one-fifth of the whole, was carbonised either in gasworks, primarily for the manufacture of town's gas, or in coke ovens, for the manufacture of metallurgical coke, in practically equal proportions. Two-thirds of the latter was carbonised in by-produce recovery plants, the remainder in the old wasteful beehive ovens.

So that, roughly speaking, we have

Total Coal Carbonised = 40 million tons		
In Gasworks	In By-product Coke Ovens	In Beehive Coke Ovens
20	13·5	6·5

At the present moment there are 8,297 by-product coke ovens built in this country, or which 6,678 are fitted with benzol recovery arrangements, capable of producing in all something like 10 million tons of coke per annum.

The yields of the various by-products obtainable on such coke-oven installations naturally vary with the locality and character of the coal-seam, but they probably average out somewhat as follows, expressed as percentages on dry coal carbonised:

District	Ammonium Sulphate	Tar	Benzol and Toluol as Finished Product
Durham .	0·9 to 1·45	2·5 to 4·5	0·6 to 1·0
Yorkshire .	1·3 to 1·5	3·5 to 5·0	0·9 to 1·1
Derbyshire .	1·3 to 1·6	3·5 to 5·0	0·9 to 1·1
Scotland .	1·4 to 1·6	3·[illegible] to 5·0	0·9 to 1·1
South Wales	0·9 to 1·1	2·0 to 3·5	0·6 to 0·75

or, to put the matter a little differently, each ton of dry coal carbonised yields from 20 to 35 lb. of ammonium sulphate, from 56 to 112 lb. of tar, and from 2 to 3½ gallons of crude benzol, &c., according to the locality. About 65 to 70 per cent. of the crude benzol is obtained as finished products (benzene, toluene, solvent, and heavy naphthas).

How rapid has been the development of the by-product coking industry in this country during recent years may be judged from the following official returns of the quantities of ammonium sulphate annually made on such plants, as compared with the corresponding quantities produced in gasworks.

Year	Tons of Ammonia Sulphate produced in	
	By-product Coke Oven Plants	Gasworks
1903 . . .	17,435	149,489
1908 . . .	64,227	165,218
1913 . . .	133,816	182,180

In the natural course of events, the final disappearance of the wasteful beehive coke-oven from this country is now only a matter of a few years; but I venture to suggest that public interest would justify the Government fixing by law a reasonable time-limit beyond which no beehive coke-oven installation would be allowed to remain in operation, except by express sanction of the State, and then only on special circumstances being proved.

There is also much need of a better and more systematic chemical control, *in the public interest, of by-product coking plants. At present, in far too many* cases, the chemists employed in coke-oven laboratories are men who have practically no chemical training other than that obtained in evening classes. And, with few exceptions, the chemist, however competent he may be, is entirely subordinated to the directing engineer, and regarded as a mere routine analyst. I can say from personal knowledge that plants which are managed and controlled by experienced chemists of broad training, combined with force of character, yield much better results than those controlled by men without such qualifications.

And even in this crisis, when so much depends on plants working, not only at their maximum output capacities but also, chemically speaking, under conditions calculated to ensure the highest yields of benzol and toluol, with a proper selection of coal, I doubt whether the measures which have been taken to advise and supervise the coke-oven industry are really adequate from the point of view of chemical control. I do know, for instance, that the experience and resources of the majority of our University Departments of Applied Chemistry, which specialise on Fuel Technology and cognate matters, have not been as fully utilised as they might and ought to have been in this connection I cannot for one moment imagine a similar state of things being permitted in Germany, where we may be sure that nothing is being left undone in the way of fully utilising all the available expert chemical and engineering knowledge which can be brought to bear on this important aspect of war munitions, and I will venture to say that, whatever may be the case in this country, in Germany at least the staff and resources of no publicly maintained Department of Fuel Technology will not be fully employed on War problems.

The coal-gas industry, which deals with some 20 million tons of coal per annum, has, especially within recent years, shown a growing appreciation of the aid of chemical science, in regard not only to the actual manufacture, but also to the domestic and industrial uses of coal-gas. The endowment in 1910 by the industry of a special Chair at Leeds University in memory of the late Sir George Livesey, of which I had the honour of being the first occupant, was a sure sign of the faith of its leaders in the value of scientific research into its special problems, and from personal knowledge and intercourse with gas engineers I can assure my chemical colleagues that any serious interest taken by scientific chemists in these problems, or in training men to tackle them, will be welcomed by the industry, no matter from what quarter such help or interest may come. For although the carbonisation of coal in gasworks is efficiently carried out, no one in the industry supposes that finality has been reached, or that existing methods and conditions cannot be improved under better chemical control.

And, moreover, the gas industry has just recently given a striking example of the public benefit which may accrue from the wholehearted co-operation of the chemist and engineer in the new nickel-catalytic process for the removal of carbon bisulphide from coal-gas, which has been worked out and brought to a successful issue by the combined skill and efforts of Dr. Charles Carpenter, Mr. D. Gibb, and Mr. Evans, of the South Metropolitan Gas Company. They have shown that the sulphur content (as CS_2) of London coal-gas can be reduced on a large scale, in regular day-to-day working, from nearly 40 drm. to about 8 grains per 100 cubic feet, without in any way deteriorating the quality of the gas, at a cost (including interest and depreciation) of 0·290*d*. per 1000 cubic feet. Such a striking success was, as Dr. Charles Carpenter acknowledges, only achieved 'because of the unrestricted and unreserved collaboration of the chemist and engineer.' Incidentally the gas industry is to be congratulated on this tacit abandonment of the old contention that coal-gas was either none the worse for the presence in it of a certain amount of sulphur impurity, or,

alternatively, if worse, that a minute amount of sulphur dioxide in the atmosphere of a living room is so rapidly absorbed by the ceiling that its harmful effects are nullified.

As the outcome largely of the work of the Joint Committee appointed in 1907 by the Institution of Gas Engineers and the University of Leeds, of which I was a member, to investigate gas-fire problems, the manufacturers of these appliances have paid much more attention than formerly to the scientific aspects of construction so far as to ensure the best combination of radiant and ventilating effects, and nearly all the larger firms have now their scientific staffs busily employed in making further advances. Prominent among the pioneers in scientific gas fire construction has been Mr. H. James Yates, who will to-morrow enlighten you as to some of the most recent improvements. I can, however, from personal knowledge, testify to the enterprise shown by most of the leading manufacturers, and that their combined efforts have resulted in a very efficient and perfectly hygienic domestic gas-fire. A Committee appointed by the Institution of Gas Engineers, upon which scientific men are largely represented, is now considering the adoption of a standard method of testing the radiant efficiencies of gas-fires, so that no one can say that the gas industry is not making every effort to put its affairs upon a thoroughly scientific basis.

Passing on to the metallurgical and allied industries, who, of course, are large consumers of fuel, there is much here to be done in improving the construction and operation of furnaces in order to check the waste of fuel, but of these details there is no time to treat, and one instance of the possibilities of very large economies as the result of scientific control must suffice. It is, perhaps, common knowledge that the most economical arrangement of plant for the manufacture of iron and steel is one in which the by-product coke-ovens, blast-furnaces, steel-furnaces, and rolling-mills are brought together on one site and under one organising direction, so that the surplus gases from the coke-ovens and blast-furnaces may be utilised to the fullest extent. My relative, Mr. T. C. Hutchinson, of the Skinningrove Iron Co., who has devoted many years of anxious thought and practical study to this important problem, ventured some few years ago to predict that with the most approved type and arrangement of plant working under strict scientific control by competent chemists it would soon be possible to make finished steel rails or girders from Cleveland ironstone with no further consumption of coal than is charged into the by-product coke-ovens for the production of the coke required for the blast-furnace, and all subsequent experience at Skinningrove has fully demonstrated that his prophecy can be fulfilled in everyday practice. Of course, it means a constant watchful control by a well-paid and competent scientific staff under efficient leadership, and in Mr. E. Bury, an old Owens College student, trained in an atmosphere of 'gas and combustion,' we have found the very man for the work.

It is, perhaps, unnecessary, even had time permitted, for me to multiply instances of possible economies in other important directions, such, for instance, as power-production and the heating of domestic apartments. There is probably no direction in which equally good results would not accrue with proper scientific application and control as those already cited as having been reached or realisable in the direction of carbonisation, or in the iron and steel industry. To-morrow we are to discuss the important subject of Smoke Prevention, in which many Manchester public men are showing an active interest, so that there will be further opportunity of referring to the matter again.

But may I, in conclusion, appeal in all seriousness to chemists and scientific men generally to take up this important matter effectively as a public duty at this crisis in the country's affairs? I would suggest that the Government be memorialised with a view to the establishment of a central organisation for the supervision of fuel-consumption and the utilisation of coal somewhat on the lines of the existing alkali works inspection which has been so beneficial to chemical industry. And in connection with such an organisation there might be undertaken a much-needed systematic chemical survey of British coal-fields, as well as experimental trial of new inventions for fuel economies. There would certainly be no lack of important work of such a properly organised Department of the State, and there can be no doubt at all that the results of its activities would be, not only a very large direct saving in our colossal annual coal-bill, but also a purer atmosphere and healthier conditions generally in all our large industrial areas.

PRINTED BY
SPOTTISWOODE AND CO. LTD., LONDON
COLCHESTER AND ETON